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CONTENTS

	PAGE
1. RECENT WORK ON MARINE METABOLISM.	I
JAS. JOHNSTONE, B.Sc.	
2. EXPERIMENTS ON ETHER-DRIFT	21
A. O. RANKINE, B.Sc.	
3. ANIMAL PARTHENOGENESIS	40
LEONARD DONCASTER, M.A.	
4. TUBERCULOSIS AMONG ANIMALS	53
W. A. BREND, M.A., M.B., B.Sc.	
5. CAMPHOR, NATURAL AND SYNTHETIC	60
PERCY A. HOUSEMAN, PH.D., A.I.C.	
6. THE TRANSFORMATION OF ELEMENTS	69
PART II. RADIUM AND ITS EMANATION.	
A. T. CAMERON, M.A., B.Sc.	
<i>(Illustrated.)</i>	
7. RECENT WORK ON CHOLESTERINE	89
C. DORÉE, M.A., B.Sc., AND J. A. GARDNER, M.A.	
8. THE BIO-CHEMISTRY OF ANIMALS AND PLANTS	106
O. ROSENHEIM, PH.D.	

9.	THE PRESENT POSITION OF THE HYDRATE THEORY OF SOLUTIONS	124
	T. MARTIN LOWRY, D.Sc. (<i>Illustrated.</i>)	
10.	THE SPIROCHILETES	148
	H. B. FANTHAM, D.Sc.	
11.	REVIEWS:	
	F. O. Bower, "The Origin of a Land Flora." L. A. BOODLE .	163
	D. H. Scott, "Studies in Fossil Botany," Vol. I. E. A. NEWELL ARDER	165
	P. Geerens, "Introduction to Metallography." W. H. MURRETT	166
	J. W. Folsom, "Entomology." F. V. THEOBALD	168
	E. P. Stebbing, "A Note on the Lac Insect." F. V. THEOBALD	169
	E. P. Stebbing, "On Some Assam Sil Insect Pests." F. V. THEOBALD	170
	Julius Thomsen, "Text-books of Physical Chemistry: Thermo- chemistry." J. C. PHILIP	170
	Fritz Haber, "Thermodynamics of Technical Gas Reactions" J. C. PHILIP	171
	N. R. Campbell, "Modern Electrical Theory." S. W. J. SMITH	172
	Francis Francis, "Pharmacology." G. T. MORGAN	174
	J. C. Willis, "A Manual and Dictionary of the Flowering Plants and Ferns." W. G. FREEMAN	175
	"A List of Official Chemical Appointments." T. A. H.	176

CONTENTS

	PAGE
1. HEREDITY AND RADIUM AT DUBLIN.	177
W. BEACH THOMAS, M.A.	
2. THE EVOLUTION OF MAN	192
J. T. CUNNINGHAM, M.A., F.Z.S.	
3. THE PRESENT POSITION OF THE HYDRATE THEORY OF SOLUTIONS	202
T. MARTIN LOWRY, D.Sc.	
4. THE TRANSFORMATION OF ELEMENTS	222
PART III. HELIUM.	
A. T. CAMERON, M.A., B.Sc.	
(<i>Illustrated.</i>)	
5. THE CLASSIFICATION OF THE PLUTONIC ROCKS	244
F. H. HATCH.	
(<i>Illustrated.</i>)	
6. ARTIFICIAL MODIFICATIONS IN THE COLOURING OF BIRDS.	265
R. LYDEKKER.	
(<i>Illustrated.</i>)	

	PAGE
7 GASEOUS REFRACTIVE INDICES	273
C. CUTHBERTSON	
8 RÖNTGEN RAYS	299
G. W. C. KAYE, B.A., B.Sc.	
<i>(Illustrated.)</i>	
9 PALÆOLITHIC RACES AND THEIR MODERN RE- PRESENTATIVES	329
W. J. SOLLAS, D.Sc., F.R.S.	
<i>(Illustrated.)</i>	
10. REVIEWS:	
J. W. W. Stephens and S. R. Christophers, "The Practical Study of Malaria and other Blood Parasite." C. W. DANIELLS	333
Arthur Turnour Murray, "Law of Hospitals, Infirmarys, Dispensaries, and other Kindred Institutions, whether Voluntary or Rate Supported." H. BRITTON	355
Hans Solereder, "Systematic Anatomy of the Dicotyledons." S. E. CHANDLER	356
John Cannell Cain, "The Chemistry of the Diazo Com- pounds." G. T. MORGAN	359
"Mines and Minerals of the British Empire." J. W. EVANS .	360

CONTENTS

	PAGE
1. THE DESTRUCTION OF WOOD BY FUNGI	361
A. H. REGINALD BULLER, D.Sc., Ph.D.	
<i>(Illustrated.)</i>	
2. THE SOUTH-EASTERN COALFIELD, ITS DISCOVERY AND DEVELOPMENT	379
MALCOLM BURR, B.A. (Oxon), F.G.S., F.L.S.	
<i>(Illustrated.)</i>	
3. THE DUCTLESS GLANDS	410
SWALE VINCENT, M.D., D.Sc.	
4. THE CHEMISTRY OF CHLOROPHYLL	425
S. B. SCHRYVER, D.Sc., Ph.D.	
5. VERTEBRATE PALÆONTOLOGY IN 1908	450
R. LYDEKKER.	
6. THE EVOLUTION OF ANIMAL FUNCTION	472
KEITH LUCAS.	

	PAGE
7. A DREAM OF FAIR HYDRONE	484
HENRY E. ARMSTRONG, D.Sc., F.R.S.	
8. PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES. PART II.	500
W. J. SOLLAS, D.Sc., F.R.S.	
9. REVIEWS:	
W. Slingo and A. Brooker, "Electrical Engineering" G. W. C. KAYE	534
Bruno Kolbe, "An Introduction to Electricity." R. W. FORSYTH	535
G. F. C. Searle, "Experimental Elasticity." A. O. RANKINE .	536

CONTENTS

	PAGE
1. DARWIN AND HIS PUBLISHER	537
JOHN MURRAY, J.P., D.L., F.S.A. (<i>Illustrated.</i>)	
2. THE EXPECTED RETURN OF HALLEY'S COMET	543
ANDREW C. D. CROMMELIN, B.A., F.R.A.S.	
3. CHLOROFORM IN USE	558
N. H. ALCOCK, M.D. (<i>Illustrated.</i>)	
4. RECENT RESEARCH ON INDIGO	575
H. H. ROBINSON, M.A., F.I.C., F.C.S.	
5. THE PHYSICAL CONDITIONS AT THE BEGINNING OF LIFE	579
CARL SNYDER.	
6. VACCINE THERAPY IN THEORY AND PRACTICE	597
D. W. CARMALT JONES, M.A., M.B., OXON.	
7. ISOMERIC CHANGE. PART I.	616
T. MARTIN LOWRY, D.Sc.	
8. THE THIRST OF SALTED WATER OR THE IONS OVERBOARD	638
HENRY ARMSTRONG, PH.D., LL.D., F.R.S.	
9. SLEEPING SICKNESS	657
G. D. PARKER, M.B. LOND.	
10. PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES	667
W. J. SOLLAS, D.Sc., F.R.S. (<i>Illustrated.</i>)	

11. THE PROBLEM OF THE PYCNOGONS	687
W. T. CALMAN, D.Sc.	
12. REVIEWS:	
C. G. Knott, "The Physics of Earthquake Phenomena." C. CHREE	694
A. Smith, "General Chemistry for Schools and Colleges." G. T. MORGAN	695
O. V. Darbishire, "A Plant Book for Schools." S. E. CHANDLER	696
A. H. Church, "Types of Fluid Mechanism." W. G. FREEMAN	697
C. Darwin, "Insectivorous Plants."	698
A. Macfadyen, "The Cell as the Unit of Life." S. B. SCHRYVER	698
F. W. Gamble, "Animal Life." O. H. LATIMER	699
"Internationale Revue der gesamten Hydrobiologie und Hydrographie." J. JOHNSTONE	700
J. A. Thompson, "Heredity." A. D. DARBISHIRE	701
M. Reeks, "Hints for Crystal drawing." G. F. HERBERT SMITH	702
A. M. Worthington, "A Study of Splashes." S. W. J. SMITH	702
C. Houard, "Les Zoocécidies des Plantes d'Europe et du Bassin de la Méditerranée." FRED. V. THEOBALD	703
E. P. Stebbing, "A Manual of Elementary Forest Zoology for India." FRED. V. THEOBALD	704
A. Berlese, "Gli Insetti." FRED. V. THEOBALD	705
Prof. Jørgensen, "The Fundamental Conception, of Chemistry." S. J. M. AULD	705
H. A. Wickham, "On the Plantation, Cultivating and Curing of Para Indian Rubber." W. G. FREEMAN	706
T. J. and W. N. Parker, "Practical Zoology." A. D. DARBISHIRE	706
J. A. Ewing, "The Mechanical Production of Cold." J. WEMYSS ANDERSON	707
E. de Wildeman, "Notices sur des plantes utiles ou intéressantes de la Flore du Congo." OTTO STAPE	708
H. Osborn, "Economic Zoology." W. E. COLLINGS	709
C. Depéret, "The Transformations of the Animal World." W. T. CALMAN	709
R. H. Aders Plimmer, "The Chemical Constitution of the Proteins." S. J. M. AULD	710
"American Chemical Journal"	711
"The Science Year Book, 1909"	711
13. INDEX	712

RECENT WORK ON MARINE METABOLISM

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IN a recent number of this journal¹ I reviewed a series of memoirs on general marine physiology, the results of which were: (1) that the density of both animal and vegetable life is greater in the polar and temperate than in the equatorial and sub-tropical seas; (2) that these variations in the density of life in the sea are to be associated with variations in the abundance of certain "ultimate food-stuffs," probably ammonia and nitric acid salts, soluble silicic acid, and phosphates; and (3) that the variations in the abundance of the first of these inorganic food-salts depend upon the activity of certain forms of marine bacteria, which possess the power of reducing nitric to nitrous acid, nitrous acid to ammonia, and the latter to elementary nitrogen. These bacteria are more active at the temperature of tropical than that of temperate seas, and in polar waters their activity is practically inhibited. Thus in the warmer seas a certain proportion of the ultimate food-stuffs available for the nutrition of the organisms of the vegetable plankton is, so to speak, thrown out of circulation; and to that extent these waters are biologically impoverished. There is actual experimental evidence for all these conclusions.

It is assumed, in such a discussion, that there is a general distinction between two categories of organisms in the sea: (1) the "producers," algæ, diatoms, other protophyta, and those unicellular organisms which have a plant-like or holophytic mode of nutrition. These organisms, by virtue of the power of photo-synthesis which they possess, can elaborate starch from carbon dioxide and water, and proteid from the carbohydrate so formed and the nitrogenous food-substances absorbed by them from their medium. Then we have (2) the "consumers," the marine vertebrata and invertebrata, as well as many of the protozoa. We usually speak of the consumers as "animals."

¹ *Science Progress*, October 1907.

They can only utilise as food-stuffs the proteid, carbohydrate, and fat which have already been built up from inorganic material by the producers. Therefore the mass of animals in the sea depends on that of the plants, while the mass of the latter depends again on the amount of the indispensable food-stuffs which stand at their disposal.

Such a distinction between plants and animals is founded on the mode of nutrition characteristic of each, and is not really invalidated by the existence of organisms which, while they are indubitably animals, yet possess chlorophyll corpuscles in their tissues, and so can elaborate starch from water and carbon dioxide by a process of photo-synthesis. For these chlorophyll corpuscles are entirely alien to the body of the animal which contains them. They originate in the "infection" of the latter by a single-celled alga, and subsequently multiply in the tissues of the animal host, or partner, of the association. We speak of such associations as symbioses, or perhaps cases of parasitism. They are to be observed in many groups of the animal kingdom—protozoa, coelenterata, flat-worms, echinoderms, polyzoa, and some mollusca. Such compound organisms exhibit two modes of nutrition which proceed simultaneously in the course of their metabolism—that of the holozooic animal, and that of the holophytic plant.

Although we may make a general distinction between the plant-like and animal-like organisms of the sea, there are nevertheless many species which cannot properly be placed in either category. Many plants are saprophytic, and not holophytic: they obtain their carbon and nitrogen from such complex substances as fatty acids or carbohydrates, and their nitrogen from compounds like urea, guanin, hippuric acid, uric acid, etc., instead of from the very simple substances, carbon dioxide, and nitric acid and ammonia salts. Saprophytes in the sea are usually moulds, yeasts, and fungi, but they are not abundant. The marine bacteria are usually saprophytes; but they also include many species which exhibit quite special modes of nutrition. Thus the sulphur bacteria can utilise, as a source of energy, sulphuretted hydrogen; nitrifying bacteria can obtain their carbon and nitrogen from carbon dioxide, and nitrous or nitric acid, and can make use of these substances as food-stuffs without possessing the power of photo-synthesis; and the denitrifying species can obtain their energy from the

reduction of the nitrogen acids. Among the true animals there are many which have a mode of nutrition which may be termed saprozoic. These are the infusoria, which live in organic liquids, and the internal parasites which inhabit the cavities of the animal body. Tapeworms possess no alimentary canal and absorb the carbohydrate or proteid, or the digestive products of the latter, to be found in the intestine or other part of the body of the host in which they live, by means of their entire body surface. A distomum does possess an alimentary canal; but there is little doubt that it also absorbs the nutritive matter of the juices of the host by means of its skin. The internal parasites of an animal thus exhibit a mode of nutrition which is very similar to that of a saprophytic plant.

It is generally assumed in biological textbooks and lectures that, apart from the mode of alimentation exhibited by the internal parasites, all animals feed in much the same way as is witnessed in the higher terrestrial forms. Food organisms are captured and ingested, and digestive processes proceed in the alimentary canal in much the same manner as in the case of the mammal, that is, proteid food substances are hydrolysed, absorbed, and reconverted into the proteid substances specific to the species considered; carbohydrates are hydrolysed to form diffusible sugars; and fats are saponified. Further, the current metabolism is usually regarded as similar to that of the warm-blooded animals; that is, energy is obtained from the oxidation of part of the tissue substance; carbon dioxide and water being excreted, along with certain nitrogenous residues; that the amount of oxygen absorbed in respiration is nearly equal to the oxygen which is present in the excretory products; and that a fairly constant respiratory quotient is the result of the metabolism of the animal.

Now the consequence of such assumptions is that the comparative anatomists have described in the invertebrata series of organs which are stated to carry out functions similar to those of the organs with similar names of the warm-blooded animal. The alimentary canal is always an organ in which the food-substances are digested and absorbed. The "hepato-pancreas" of a mollusc or other invertebrate is always a gland in which glycogen is elaborated, and in which enzymes are prepared; enigmatical structures in connection with the alimentary canal, such as the crystalline style of the mollusca, are reserves of food

material, or contain digestive ferments. The ctenidia of a mollusc, the adaptive gills of a limpet, the branchial leaflets, respiratory plumes, cerata, etc., of other mollusca, the tentacles of a tube-forming worm, the branchiæ of an ascidian, and other structures, are all supposed to be organs in which oxygen is absorbed from solution in sea-water, and from which carbonic acid and other waste substances are given off to the surrounding medium. When special apparatus for the respiratory function does not exist, it is usually supposed that this function is carried out by the entire surface of the skin. Sometimes, as in the case of the lower crustacea, it has been stated that the pumping movements of the rectum which may often be observed in these organisms are concerned with the respiratory process, water containing oxygen being taken into the alimentary canal, and absorbed by the internal surface of the latter. "Eyes" are always visual organs. So also with many investigations into the comparative physiology of the invertebrata. Proteolytic, amylolytic, and lipolytic ferments have been isolated from the alimentary canal and its glands; and since the reactions of these substances *in vitro* are similar to those of the digestive ferments of the mammal, it is assumed that the metabolic processes are also similar.

Immediately we assume that the marine invertebrata possess organs which carry out functions essentially similar to those of the warm-blooded animal we encounter difficulties with regard to their alimentation. It is true that many carnivorous species which live in the sea may be very similar to the mammal with respect to their general metabolism, but we cannot suppose that this is universally the case with the free-living animals. We find it usually stated, for instance, that the gills of a lamelli-branch mollusc such as the mussel are organs which serve for respiration, and also for the establishment of a current of water into the mantle cavity, which current carries with it the food organisms. These food organisms are strained out by the meshwork of the gill filaments, are caught up by the labial palps, and are taken into the mouth. Then digestion and assimilation proceed presumably in much the same manner as in the higher animals. I do not know of any estimations of the respiratory surface of the gills of such a mollusc, but one is impressed with its magnitude in comparison with the mass of the body of the animal. So also with an ascidian, where the surface of the branchiæ is a relatively large one. Yet these

animals are almost entirely sedentary in their habits, and one fails to see the necessity for such an intensity of oxidation as would be indicated if we supposed that the function of the gills is that of the absorption of oxygen from the sea-water. Then when we examine the alimentary canal in order to determine what are the characteristic food organisms we often have difficulty in finding that such are really present. The intestine of a cockle, or a lugworm, is filled with sand and mud, but among this one finds very few food organisms; certainly the latter are not present in such number as would justify us in assuming that they formed the entire food supply of the animal. Yet one is impressed with the enormous food contents of the stomachs of many carnivorous marine animals, the cod and herring, for instance; and in these animals the gill surface is not so great relatively to the general surface of the body as in the sedentary mollusc, where the food contents of the intestine are not at all obvious. So also in other cases. Indeed we know very little about the food, or the feeding habits, of many marine animals, and what knowledge we do possess relates principally to the fishes and other animals of economic importance, for in such cases bionomical investigations have a certain practical value. Most zoologists who have written comprehensive memoirs on marine animals say, as a rule, very little about the food of their types; and their attitude towards the latter is often that of the Cambridge man to whom an animal was only an animal when it was dead and preserved! So, just because the living organism is not always studied, suggestions as to the purpose of organs must often have been neglected, and functions have usually been interpreted from the results of morphological comparisons. There can be no better example than that of the eyes of *Pecten*. The structure of these organs suggests irresistibly that their function is a visual one, and no doubt this is the view that is taught in lecture-rooms and laboratories. Yet one is inclined to doubt this when he reflects on the possible reasons for the possession of such a battery of highly complex eyes in an animal which is not predatory; which (in its adult phase at least) is not greatly preyed upon by other animals; and obtains its food in a perfectly automatic manner, just as does the mussel, where the eyes are practically non-existent. So Patten suggested that the eyes were really organs for the utilisation of solar energy; and though this source of energy is utilised by more

than half of the total mass of terrestrial organisms, Patten's suggestion has generally been regarded as rather fantastic and irresponsible.

Further, we know that some marine animals may subsist for quite lengthy periods in aquaria without visible food supplies, and this is also the case in the sea. One hardly ever finds food in the alimentary canal of a plaice during the months of December to February—a fact well known to fishermen, who often express surprise when they find a plaice which does contain food in its intestine during the latter part of this period. The fish during these winter months decreases in weight, and the loss is suggested in the formula:

$$\text{Weight (in grammes)} = \frac{\text{Length}^3 \text{ (in centimetres)}}{100} \times \text{constant,}$$

the constant varying from 1 to 0.8.

Now, does the fish actually exist without food during these months, and does the loss of weight correspond with the metabolic exchange which would be observed if we were to investigate its physiology? We remember that it is a "semi-sedentary" animal; that its respiratory movements are slowed down during the cold months; and we conclude that it takes no food, passes through a kind of hibernation phase, during which reserve food materials are utilised. Nevertheless, we find that some invertebrates do appear to "starve" during certain periods, but probably subsist upon dissolved food matters, and it is necessary to prove, by actual measurements of the metabolic exchange between the animal and its environment, that no such food sources are really drawn upon. We find, however, that there are very few such studies in the literature of comparative physiology.

We may consider two instructive instances of the metabolic processes that proceed in the alimentation of invertebrate animals. A holothurian (or sea-cucumber) is usually stated to be a predatory animal. Some species appear to live on solid ground at the sea-bottom, and as they move about on the latter they gather up food particles with their buccal tentacles, or feelers, and brush off the latter into their mouths. Some live at the bottom of deep seas, in the ooze which forms the ocean floor; and as they move about they swallow this ooze for the sake of the organic remains which it contains. Others, such as *Cucumaria*, are said to be distinctly predatory, catching and eating other animals. In these species the buccal feelers are long,

branched, and covered with adhesive mucus, and the animal employs them to sweep through the water and capture pelagic organisms which come into contact with them. The feeler, with the attached food organisms, is then inserted into the mouth, and the latter being tightly closed the feeler is withdrawn forcibly, so that the food particles are retained. Some apparently burrow in the mud, swallowing the latter like an earthworm, and extracting the included nourishment. Glands secreting digestive juices are described. The alimentary canal has been divided into œsophagus, stomach, intestine, and rectum. Muscles are described which are said to produce peristaltic movements of the viscus, with the object of mixing the food contents and the digestive juices. Apparently, then, we have to deal with animals which nourish themselves in essentially the same manner as in the case of the warm-blooded creatures. So also with the function of respiration. This is said to be carried out by pumping movements of the rectum (just as some micro-crustacea are said to breathe), water being taken in and forced out of the anus. Opening into the cloaca are the "respiratory trees," long, branched tubes, ending in fine-walled ampullæ. The inspired water is said to be circulated through this system of vessels, and is said to diffuse through the walls of the ampullæ, carrying the dissolved oxygen into the coelomic fluid. Here, then, there is an apparatus to which is ascribed a function essentially similar to that subserved by the gills of a fish, or the lungs of a mammal—the interchange of oxygen and carbon dioxide between the tissues of the animal and the surrounding medium.

These views are based on the resemblance which the structures in question—the alimentary canal and its glands, and the respiratory trees—bear to the alimentary canal and its associated glands, and to the lungs of a mammal. But does actual physiological investigation bear out the interpretation which is usually made as to the function of the organs of the holothurian? Pütter,¹ in a series of papers of very considerable interest and importance, has investigated the exchange of oxygen, carbon, and nitrogen in the case of the holothurian, *Cucumaria grubei*,

¹ "Studien zur vergleichenden Physiologie des Stoffwechsel," *Abhandl. königl. Akad. Wissensch. Göttingen; Math-Phys. Klasse*, Bd. VI. Nro. 1, 1908; "Die Ernährung der Wasserthiere," "Der Stoffhaushalt des Meeres," *Zeitschr. für allgem. Physiologie*, Bd. VII. 2 and 3 Heft, Jena, 1907.

and his results certainly seem to afford good grounds for refusing implicitly to accept the accuracy of the views commonly taught. He was able to keep the animal in an aquarium for six months without any visible food. During this time its weight decreased, and it was easy to ascertain, from a knowledge of the composition of the body, and from the weight of the latter at the beginning and end of the experiment, how much carbon, etc., had disappeared—that is, had been given off to the medium.

Pütter also estimated the average rate of exchange of oxygen, carbon, and nitrogen during the period of "starvation." The chemical methods employed appear to me to be quite sound.¹ Oxygen was estimated by the volumetric method involving the oxidation of manganous to manganic chloride. Carbon dioxide in solution was estimated by boiling the water sample in the presence of acid and in a stream of CO₂-free air, the gas being afterwards absorbed and weighed. The total carbon was determined by oxidising the water sample by potassium dichromate and sulphuric acid. The CO thus formed was oxidised to CO₂ by passing it over glowing copper oxide, and the latter gas was absorbed and weighed. Nitrogenous substances were estimated by Kjeldahl's method; or by simple distillation of the water sample, when volatile bases were obtained; and by the usual methods for the determination of nitrates, nitrites and ammonia compounds. Pütter thus estimated the rate of exchange during the period of "starvation." The average loss of carbon during this period, estimated from the loss of weight of the animal, was 0.0155 mg. per animal per hour. But, nevertheless, the actual amount of carbon given off by *Cucumaria* during the same six months was no less than 0.2 mg. per animal per hour—that is, more than ten times as much as the loss due to the decrease of weight during the "starvation."

A sponge is an organism which displays but little differentiation of tissues. It is strictly sedentary, and the only movements which may be observed to be carried out by it are insignificant contractions of the oscula. It possesses no prehensile organs, and it obtains its food in an automatic manner from the current of water which circulates through its system of cavities. Although presenting very great modifications in arrangement this system is essentially similar in all sponges: there is an inhalent system of openings, an exhalent system, and a series of

¹ Vergl. *Phys. Stoffw.* p. 6.

chambers placed between the two. The internal walls of these chambers are lined with "choanocytes," cells which are provided with prominent collar-like structures, and with long flagella. The lashing of these flagella produces a difference of pressure which results in the establishment of a current of water which enters the canal system of the sponge through a series of pores called ostia, and leaves through other pores called oscula. It is obvious that the substances utilised by the animal as sources of nutritive material must be conveyed to it by this current of water.

It has usually been assumed that the substances which serve as the sources of food of the sponge are plankton organisms such as diatoms or protozoa suspended in the water circulated through the canal system; and it has been supposed that the latter are ingested by the choanocytes in much the same manner as a diatom is ingested by a protozoan, such as an *Amoeba*. Many series of experiments have been made in order to determine the precise nature of these food organisms, and the details of their capture and digestion by the sponge. Usually the animal has been kept in water to which was added suspended particles such as carmine or indigo, and the passage of the latter through the canal system and their further fate has been traced. Most of these experiments seemed to indicate that solid food particles, such as plankton organisms, contained in the circulating current of water, form the food of the sponge, and that these are captured by the choanocytes, and absorbed and assimilated in the dermal cells. Altogether the results of such experiments do not seem to have been very successful, that is, from the point of view from which they started—that the sponge fed on solid suspended food organisms. Haeckel came near the truth when he suggested that sponges feed on the insoluble or soluble products of decomposition of other dead organisms; and Loisel probably came nearer still when he showed that a sponge might live in filtered water to which was added the juices obtained by breaking down the bodies of other sponges.

Thus the commonly accepted view has been that a sponge finds an adequate food supply in the plankton organisms present in the water current passing through its canal system. Now before we can regard this as established it is necessary to ascertain what are the food requirements of the animal, and whether or not the plankton present in the volume of water

circulated is sufficient for these requirements. It does not appear to me, when Pütter's experiments are considered, that this view is at all tenable. The actual amount of oxygen, carbon, and nitrogen exchanged between the sponge *Suberites domuncula* and its surrounding water was determined by Pütter,¹ and it was found that for an animal of about 60 grammes in weight, and about 60 c.c. in volume, this interchange per hour was :

0.92 mg. of carbon, and
0.0138 mg. of nitrogen.

Let us suppose, for the moment, that these amounts of carbon and nitrogen are to be obtained from plankton organisms. It is then necessary that we should know what are the proportions of such food substances present in the sea, and available for those animals which presumably utilise them as food. What quantity of plankton then was present per unit volume of the water of the Bay of Naples (where Pütter made his experiments)? The results of the latter would have been more satisfactory if quantitative plankton observations had been carried on concurrently with the metabolic researches. Such observations were carried out by Schütt and Apstein² (1888-96), but they were made by means of the Hensen vertical net, and it appears from the investigations of Lohmann³ that this apparatus does not present us with an accurate picture of the contents of the sea so far as the smaller planktonic organisms are concerned. The only results which can be applied are those obtained by Lohmann³ in a series of experiments made in the open Mediterranean off Syracuse, and these are of particular value since they were made by various methods, and probably represent very approximately the total contents of the sea *in situ* in organised food substance. It is indeed possible that the abundance of plankton in the Mediterranean off Syracuse may differ from that in Naples Bay, but we shall see that a considerable difference may be assumed without affecting the conclusions to be drawn. Lohmann found as the results of an exceedingly careful series of plankton fishing experiments

¹ *Vergl. Phys. Stoffw.* p. 43.

² Summarised by Brandt in "Stoffwechsel im Meeres," *Wissensch. Meeres untersuch. Kiel Kommission*, Bd. vi. Abth. Kiel, 1906.

³ "Neue Untersuch. Reichthum d. Meere an Plankton," *ibid.*

RECENT WORK ON MARINE METABOLISM 11

that 1 cubic metre of sea-water in the sea off Syracuse contained :

Metazoa	17,325
Protozoa	325,510
Protophyta	2,082,860
Bacteria	785,000,000

These numbers represent a total volume of about 53·63 c.c. of plankton organisms, and a total weight of about 55·6 milligrammes. Utilising Brandt's tables in order to convert this mass of plankton organisms into food values, we find that the total mass of carbon in 1 cubic metre of sea-water was :

Metazoa	2·48 mg.
Protozoa	0·08 "
Protophyta	1·22 "
Bacteria	0·06 "
Total	<hr/> 3·84 mg.

And again making use of Brandt's tables¹ of the composition of planktonic organisms, it is found that the dry organic matter in 1 litre of sea-water contained :

Carbon as proteid	0·00129 mg.
Carbon as carbohydrate and fat	0·00255 "
Total carbon	<hr/> 0·00384 mg.
Total nitrogen	0·00039 "

Thus 1 litre of water contained 0·0038 mg. of carbon in the form of microscopic animals and plants. If then we assume that the sponge obtains its food from such a source we can easily calculate what quantity of water must be circulated through its canal system in order to supply its requirements. We only have to divide 0·92 (the mass of carbon required) by 0·0038 (the mass of organised carbon present per litre) in order to find that the sponge must pass through its canal system about 242 litres of water per hour, or some 4,000 times its own volume. And in so doing we assume that every plankton organism is removed from this volume of water by the choanocytes of the flagellated chambers—an exceedingly improbable assumption.

¹ "Chemische Zusammensetzung des Planktons," *Wiss. Meeresunt. Kiel Kommission*. Bd. iii. Abth. Kiel, 1898.

But it is possible to calculate how much sea-water does actually pass through these flagellated chambers. The diameter of the osculum is about 6 mm., and the velocity of the exhalent stream of water was estimated by Pütter as about 5 mm. per second.¹ Thus the volume of water circulated is only about $\frac{1}{2}$ litre per hour, assuming that the stream is a constant one, which is not the case, for the osculum is often closed. If, then, the sponge obtains its food-stuffs from the organisms of the plankton, it can obtain, at the very most, only about 0.0019 mg. of carbon per hour that is, about $\frac{1}{105}$ th part of the carbon it actually excretes! There does not seem to be any other conclusion than that the mass of food organisms in the plankton is quite inadequate for the nutrition of the sponge; and this conclusion would be justified even if we admit that there may be some considerable errors in the estimations of the density of the plankton, and in the estimation of the rate of circulation of the water passing through the canal system of the sponge.

How, then, do these two animals obtain their food? There does not appear to be any other conclusion than that they make use of some source of food other than that afforded by the organisms of the plankton, or of the ocean floor. Such sources of food do exist, but before we proceed to consider them it may be pointed out that the processes of alimentation in the sponge and holothurian studied by Pütter differ strikingly from those characteristic of the warm-blooded animal. In the latter, energy is obtained by the oxidation of certain constituents of the tissue substance, and there is a certain relation between the oxygen absorbed in inspiration and the carbon dioxide excreted. That is, the respiratory quotient, CO_2/O_2 , is a tolerably constant ratio, and indicates that the principal katabolic process by which the energy of the organism is obtained is an oxidation one. Now so long as we assume definite respiratory functions for such organs as gills, ctenidia, etc., and definite digestive and absorptive function for the alimentary canal and glands, and assume further that these are the sole functions performed by the organ systems mentioned, it seems natural to suppose that the relation between the inspired oxygen and the excreted carbon dioxide will be similar in value and constancy to that exhibited by the warm-blooded animal.

¹ *Vergl. Phys. Stoffw.* p. 43.

But this is certainly not the case. Vernon¹ showed in 1895 that the respiratory quotients in the lower invertebrata varied remarkably, and in the more recent studies made by Pütter the same facts are disclosed. Thus in a series of determinations of the exchange of oxygen and carbon dioxide in the sponge *Suberites* the following figures were obtained, at a temperature of 13.5° C.:

Oxygen absorbed = 30 mg. per kilo. per hour ;

Carbon dioxide excreted = 282 mg. per kilo. per hour ;

from which the respiratory quotient is 6.5, an astonishingly high ratio.² So also in the case of the holothurian *Cucumaria*, when the following values were obtained :³

Temperature.	Oxygen absorbed.	Carbon dioxide excreted.	Respiratory quotient.
11.7° C.	27	140	3.8
13.7° C.	33	166	3.68
17.0° C.	60	211	2.55

Thus there can be no doubt that katabolic processes occur in the metabolism of these animals which must belong to a type other than the purely oxidation one, and we are compelled to regard these processes as analogous to those characteristic of the saprozoic animal or saprophytic plant. It is well known that many bacteria are anærobic, and obtain their energy from decomposition or fermentation processes which take place in the complete absence of oxygen. Infusoria also live in media which contain much organic matter, such as carbohydrate, decomposition products of proteids, extractives, etc., and comparatively little oxygen. So also in the case of the nutrition of the internal parasites of animals. Quite apart from the above evidence, it would be reasonable to expect that among the lower invertebrata living in the sea there would be some which would display a similar or analogous mode of nutrition, and this conclusion is that arrived at by Pütter. From a consideration of the above figures, and assuming certain types of decomposition of proteids, fats, and carbohydrates, he gives

¹ "Respiratory Exchange of the Lower Marine Invertebrata," *Journ. Physiology*, vol. xix. 1895-6.

² *Vergl. Phys. Stoffw.* p. 33.

³ *Ibid.* p. 52.

the following scheme for the derivation of the energy of the sponge *Suberites*:

	Proteids.	Fats.	Carbohydrates.
Energy obtained by fermentation reactions (in calories) . . .	8	2	282
Energy obtained by oxidation reactions (in calories). . .	76	63	0

From this scheme it appears that the animal obtains about 32 per cent. of its energy from oxidation reactions, and about 68 per cent. from processes of the fermentation type.¹

It will be seen from the above figures that the intensity of metabolism in the animals studied is notably influenced by change of temperature, increasing with increase of the latter factor. This is, of course, what we would expect, but the figures also show that the character of the reactions involved also changes with the temperature; thus the respiratory quotient falls as the temperature rises, indicating that in the latter condition the oxidation process becomes more prominent. Another of Pütter's results is much more unexpected: he found that the intensity of light also affected the rate of metabolism. In a series of estimations of the rate of absorption of oxygen the following figures were obtained²:

Temperature.	In the dark.	In the light.
12°3 to 12°5 C. . .	43	57
12°8 to 13° C. . .	68	131
17°9 to 17°7 C. . .	15	41

Thus something analogous to the photo-synthetical reaction of the typical green plant is also displayed by the sponge. It is true that photo-synthetical reactions are carried out by many animals, such as corals, alcyonaria, turbellaria, and isolated species among other groups, but in such cases we can trace the formation of starch from carbon dioxide and water to the included algal corpuscles which arise by the infection of the tissues of the animal by the pelagic cells of *Zoochlorellæ*. It is indeed not impossible that the sponge investigated by Pütter may have been infected in some such

¹ *Vergl. Phys. Stoffw.* p. 42.

² *Ibid.* p. 31.

manner, but this was apparently considered and regarded as unlikely. It is also possible that changes in the intensity of light may affect the sense organs on the surface of the body, and by producing changes in the amount of movement, so indirectly affect the rate of metabolism. The sponge is a sedentary animal quite incapable of locomotion, and the only mechanical work performed by it appears to be the maintenance of the current of water circulating through the canal system. Possibly light exchanges may affect the velocity of this current, and so indirectly the intensity of metabolism. But the evidence seems to point to a direct influence of changes in the intensity of the incident light on the metabolism of the organism.

Thus certain conclusions, which appear to me to be quite novel, emerge from a consideration of the observations now under review: (1) that sponges and cucumarians obtain their food otherwise than by capturing and digesting living organisms; (2) that the intensity of metabolism in some sponges, at least, is directly influenced by changes in the intensity of light, so that photo-chemical reactions are carried on in these organisms just as in a green plant; and (3) that the mode of nutrition of some of the lower invertebrates differs strikingly from that of the higher animals, and is to be compared with that of the saprophytic plants.

What is the nature of the food substances utilised by the sponge and holothurian, and probably by many other of the lower marine invertebrata, such as ascidians, some molluscs, deep-sea fishes, and probably many other animals? It appears to be very probable that these food-stuffs are present in solution in the sea in the shape of organic compounds such as extractives, carbohydrates, organic acids, and substances akin to the humus compounds present in the soil. I have already shown that recent investigations indicate that certain inorganic salts, present in solution in the sea, are of first-rate importance for the nutrition of the lower organisms of the plankton, and consequently for the animals which feed upon the latter. We must recognise now that animals may also feed upon substances dissolved in the sea.

So far there is not much evidence that these organic compounds are universally present in solution in the sea in such proportion as to constitute an abundant food supply for saprozooic animals. Pütter has indeed shown that such

compounds are present in considerable proportion in the Bay of Naples within rather close proximity to the land.¹ We know that a considerable quantity of carbon dioxide is present in solution, but there are also other carbon compounds. One litre of water in Naples Bay contained:

Carbon dioxide	99 mg.
Volatile organic acids	36 „
Carbohydrates, higher organic acids, etc.	70 „

Here there is a considerable food supply, and a review of the evidence afforded by Pütter's experiments appears to justify us in regarding these carbon compounds as food-stuffs for such animals as *Suberites* and *Cucumaria*. We have seen that the sponge must pass about 242 litres of water per hour through its canal system in order to obtain sufficient food, and that it is impossible that it can circulate this volume of water. But it can pass about $\frac{1}{2}$ litre of water per hour through its osculum, and taking the above figures as representing the average composition of the sea-water, so far as concerns soluble carbon compounds, it is evident that this latter volume is enough if we suppose that these substances can be utilised as food-stuffs. For $\frac{1}{2}$ litre contains about 50 mg. of carbon in the form of soluble compounds other than carbon dioxide, and only 0.92 mg. of carbon is required for the hourly metabolism of the sponge. So even if we assume that not all these carbon compounds present in solution in the sea are suitable food-stuffs, and if we assume that the coefficient of absorption is not a very high one, it may still be the case that the sponge finds sufficient nutriment in the dissolved food-stuffs.

These soluble carbon compounds probably originate as products of the katabolism of the protophyta and bacteria of the plankton. These are the smallest organisms found in the sea, and their mass is to be regarded as small compared with that of the larger planktonic organisms, such as the larger protozoa, micro-crustacea, or the coelenterates and worms. But the conclusion is not so strange as it at first appears to be when we reflect that the protophyta and bacteria are much more numerous than all the other groups of animals which make up the marine plankton, and that the intensity of metabolism in their

¹ *Stoffhaushalt des Meeres*, p. 328.

case is greater than in the case of the larger organisms. Bacteria and protophyta absorb food solutions over their entire surface, and while the mass decreases with the cube of the radius, the surface decreases as the square of the radius. Thus if we take the surface of a single bacterium to be approximately $10\mu^2$, we find¹ that 1 kilogramme of dry bacterial matter corresponds to a total surface of about 62,500 square metres, while in man 1 kilogramme of dry organic matter corresponds to a surface of only 0.168 square metre. If we suppose that the general proportions in which the various groups of organisms are present in the plankton are represented by Lohmann's figures (which I have quoted above), it is possible to calculate the aggregate surface of the organisms belonging to each group. Putter gives the following figures :

Protozoa	291
Metazoa	1,480
Bacteria	4,000
Protophyta	4,229

the total surface being represented by 10,000.

If then the intensity of the metabolism is proportional to the absorptive surface, it follows from the above figures that about 82 per cent. of the total metabolic changes of the plankton is due to those of the protophyta and bacteria. The latter groups of organisms are therefore those which contribute in greatest proportion to the formation of the carbon compounds present in solution in the sea, which we have reason to believe are utilised by some at least of the lower invertebrata as a source of food.

If a considerable proportion of the energy of some of these animals is derived from the fermentation of carbon compounds absorbed from solution in sea-water, we may ask what are the precise functions of the organ systems usually termed digestive and respiratory. It is evident that there cannot be a clear and undoubted distinction in function between these two series of structures. If Pütter's results should prove to be accurate, it is apparently the case that organs to which a respiratory function is usually ascribed are rather to be regarded as, in general, apparatus for the absorption of dissolved nutritive substances, no less than organs by means of which oxygen is absorbed from

¹ *Stoffhaushalt des Meeres*, p. 343.

the sea, and carbon dioxide given off. Thus we shall be compelled to regard the ctenidia of some mollusca, the respiratory trees of holothurians, the branchial sacs of ascidians, the respiratory plumes of some worms, and other structures, such as the appendages of cirripede crustacea, as organs which have the function of taking up dissolved food substances from the surrounding sea-water. The acceptance of such a view will probably clear up some difficulties which have presented themselves to the zoologist as the result of a too complete reliance on the conclusions of comparative anatomy. Why, for instance, should some sluggish and inactive animals possess respiratory organs with an extraordinarily large surface; or why should many invertebrates possess a richly branched gut; or why again should a typical alimentary canal exist in some animals while it is usually very difficult to demonstrate the presence of solid food particles in the lumen of the latter? The cod, an active animal, has gills which have not nearly such a large respiratory surface, relatively to the mass of the animal, as the ctenidia of the sedentary oyster. Pycnogonids possess a richly branched gut, but the alimentary canal of most fishes is relatively much simpler and has an internal surface much less in proportion to the mass of the animal. It is very difficult to find sufficient food particles in the gut of some lamellibranch mollusca to satisfy oneself as to the materials on which these animals subsist. So also with the pycnogonids, and with other marine invertebrata.

If we assume, however, that a proportion of the food of these creatures is to be derived, not from other organisms taken into the gut and digested, but from carbon (and nitrogen) compounds absorbed by the integument of the gills, or other "respiratory organs," or by the general surface of the body, or by the epithelium of the gut, such difficulties disappear. We need not suppose that the alimentary canal is purely an organ for the absorption of dissolved nutriment. It may be that this was its primary function and that it was evolved merely as an extension of the general absorptive surface possessed by the animals in which it first appears. The ingestion of solid food may co-exist with the absorption of dissolved food, just as in insectivorous plants a part of the nutritive substances required by the organism may be obtained by the digestion of the bodies of insects, while the major part of the food of the plant is obtained by a photosynthetic process. In the carnivorous marine animals what

was originally a secondary function of the gut has now become the principal, or even the sole one. It is well known that it is very difficult to find solid food particles in the coelenteric cavities of the true corals. It is true that the conditions of nutrition are very complex in the case of these animals since zoochlorellæ may infect the tissues of the coral zooids, and may exercise the power of photo-synthesising carbohydrate. But it is also possible that the corals may feed by the absorption of dissolved carbon and nitrogen compounds.

Finally, I may refer to some provisional conclusions of Pütter's with regard to the variations in density of marine life in the warm and cold seas. It may be regarded as established that the polar and temperate seas are richer in plant and animal life than are the equatorial and tropical regions of the ocean.¹ The cause of this is traced by Brandt to the activity of marine denitrifying bacteria, which is greater at high than at low temperatures. Since these organisms reduce nitric and nitrous acids and ammonia to elementary nitrogen, it follows that more of these indispensable food-salts must be destroyed in the warm than in the cold seas, and that consequently plant life in the former areas must be impoverished. But Pütter suggests that this difference in the density of animal and plant life in the two opposed areas is due to the different shares taken in the total metabolism of an organism by types of metabolism which he terms "Betriebsstoffwechsel" and "Baustoffwechsel." The former is the current metabolism of the organism, the processes by means of which its ordinary energy is obtained. The latter is the structural metabolism, as the result of which the organism produces new tissue in the form of eggs, embryos, buds, etc. At the higher temperatures of tropical and subtropical seas the current metabolism is, relatively to the mass of the organism, much greater than at the lower temperature of polar or temperate waters. We may regard the current metabolism in the warm seas as more wasteful than in the colder regions. Therefore, less of the food-stuffs taken up by the organism from the surrounding medium, in warm seas, can be devoted to the building up of new individuals by the ordinary processes of repro-

¹ Even if they are not richer in life, it is still the case that the density of animals and plants is not less in polar than in tropical seas. It ought to be less (because of the lower temperature and deficient lighting) if some other factors were not involved.

duction than in the colder waters where the current metabolism is relatively less ; and consequently the density of individuals is less, although the total mass of food-matter in the process of exchange may be approximately similar in the two cases. It is true that the intensity of both current and structural metabolism increases with the temperature, but the increase is more rapid in the former than in the latter case.

EXPERIMENTS ON ETHER-DRIFT

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THERE are few branches of experimental inquiry which have so uniformly failed to disclose definite information as that which has aimed at determining, by wholly terrestrial methods, the motion of the earth through space. It is, of course, possible, by means of astronomical observations, to find out how the earth moves relatively to the sun or other celestial bodies; but there is no reason for regarding any one of these, more than another, as fixed. No meaning can be attached to numbers specifying the movements of a body, unless the system with reference to which measurements are made is also known. The selection of the particular system of reference is a matter of choice; nevertheless, when considering stellar and planetary motion, it is surely desirable to choose something less arbitrary than the so-called fixed stars. It has become customary to regard the ether as a suitable and most natural standard structure relatively to which all motion of matter should be defined. The nature of the ether is at present, in the main, hypothetical. It is postulated in order to account for the propagation of light and other electro-magnetic waves through space; but of its actual structure and the exact method by which these effects are carried through it, nothing is known with certainty. It is sometimes convenient to think of it as a frictionless fluid filling all space, or, at other times, to suppose it possessing the properties of a perfectly elastic solid.

These assumptions may, or may not, be true. If its mere existence be admitted, however, it may be described as "at rest," provided that there is no relative movement between the parts of which it consists. This may not, indeed, be the case. There is considerable evidence of modification of ether in matter, and possibly in the near neighbourhood of matter. Such modification may amount to a difference of velocity between the parts of the ether affected by the presence of

material bodies and the parts not so affected. If relative motion of this kind exists, it is obviously impossible still to take the ether, as a whole, as our standard system of reference. A particular part of it must then be selected. Now, it is extremely unlikely that those parts of the ether which are very remote from all material bodies are in any way disturbed by them, and they can be conveniently defined as stationary. With respect to this free ether, the earth will, in general, be moving. The possibility of determining its motion by terrestrial observations alone will, however, depend on whether the ether in the near neighbourhood of the earth is free or not. For suppose that it is not free—that is, that it is partially, if not wholly, dragged with the earth in its course—then all that could possibly be measured on the earth itself would be the relative motion between it and the neighbouring ether; and this would afford no information as to what may be called its “absolute” motion, *i.e.* its motion relative to free ether. If, on the other hand, the ether is not modified near the earth and drifts past it as it moves onward, it at first sight seems likely that the velocity of the earth in free ether could be determined without the aid of astronomy.

The experiments which form the subject of this paper are based upon the assumption that the latter is the case, in the hope that they would afford a means of discovering the resultant motion of the earth through space, and, incidentally, of testing the accuracy of the astronomical estimate. The justification for these attempts is principally based on the well-known phenomenon of the aberration of the stars, first observed by Bradley about 1725. Briefly stated, it is that the stars appear to occupy positions in the heavens different from the calculated positions at various times of the year. Stars on the ecliptic appear to describe straight lines, those at the pole of the ecliptic small circles, and intermediate stars small ellipses about their true positions. These apparent movements can be explained very simply indeed, if it be assumed that the ether near the earth is perfectly free, but no satisfactory explanation has yet been offered accounting for aberration on the supposition that the ethereal medium is dragged either partially or wholly by the earth in its neighbourhood. Stellar aberration may be taken, therefore, as at least suggesting that the ether drifts freely past the earth. It is true that an equally good explanation

could be given on the assumption that the whole of the ether is carried with the earth, and freely passes the star under observation; but such a conception is obviously absurd. In truth, it probably drifts past both earth and star; and it is important to remember, in this connection, as has been shown by Lodge,¹ that all that the aberration angle measures is the relative motion of the source and the receiver, and not the total movement of the latter with reference to the medium. A uniform drift of the ether past both star and observer would produce no variation in the magnitude of the observed aberration. Hence, although this observation may be taken as a guide as to the size and direction of the earth's motion, the total motion may be considerably different; and a successful experiment of the type to be described would measure the whole drift of the ether.

Considerations of this kind are sufficiently weighty to justify completely the attempts which have been made to determine this motion; but, as has been already pointed out, none of them has succeeded in throwing light on the main question. They have only afforded ground for speculation as to their non-success—interesting and important speculation, it is true, but, nevertheless, only speculation. Let us now proceed to consider in detail the experiments themselves, and also some suggestions which have an important bearing on the subject.

In the first place, it will be well to consider what is known from an astronomical point of view regarding the movement of the earth. Besides rotating on its axis, it is moving in a nearly circular orbit round the sun as centre, with a velocity of about 19 miles per second. In comparison with this, the rotary motion of the earth is negligibly small, only amounting, at the equator, to about $\frac{1}{60}$ of the above value. If, then, the ether be supposed stationary relative to the sun, the velocity with which it is to be expected to drift past the earth amounts to 19 miles per second, or very nearly one ten-thousandth part of the velocity of light. Now, although the rotation of the earth does not considerably alter the magnitude of this velocity relative to an observer at a particular point on the earth's surface, it does produce marked diurnal variation of its direction. This direction will obviously at any time be in the plane of the ecliptic—*i.e.* the plane of the earth's orbit—and perpendicular to the line

¹ O. J. Lodge, "Aberration Problems," *Phil. Trans.* vol. clxxiv, 1893.

joining the centre of the earth to the sun. The earth, in turning on its axis, causes the drift to be twice a day horizontal at points on the earth's surface within certain limits of latitude. Thus, in this latitude, the motion of a point relative to the sun is horizontal and due east and west at 12 o'clock on June 21, day and night. At other times of the day and year various angles are made with this direction. These variations differ for different latitudes, so that they must be calculated for each place at which experiments on the drift of the ether are carried out.

But is there any reason for supposing that the ether is at rest as regards the sun? Long-continued observations of the stars point to the conclusion that the sun itself has proper motion relative to these stars. There is evidence that it is moving with a velocity, not very different in magnitude from the earth's orbital velocity, towards the constellation Vega. If this be the case, the motion of the earth relative to those stars the observations of which lead to the above conclusion is the resultant of both velocities. The direction of this, also, will vary with latitude, time of day, and time of year; and there is the further consideration that the magnitude will depend largely on the time of the year. Taking the sun's proper motion as being 15 miles per second in the direction indicated, it will be found that twice a year—in March and September—the earth's orbital motion and the sun's proper motion have nearly the same direction; being, however, in agreement in the former case and opposed in the latter. From this point of view, the most favourable time of the year for experiments on the measurement of ether-drift is the month of March, at one period of which a velocity of about 31 miles per second is reached. But it must be remembered that this velocity is merely that of the earth relative to a particular set of stars which may themselves be moving with reference to the ether. Astronomy can only measure motion of bodies visible through the most powerful telescope for the time being in existence. Is it not possible, and, indeed, probable, that the whole visible universe is moving with respect to the ether? This is the question which the following experiments have aimed at answering; and the magnitude of the astronomically determined velocity has been merely a guide for fixing the sensitiveness necessary in the particular apparatus used.

How then, supposing the ether near the earth to be free, is the velocity of its drift past the earth to be measured? Ether

is the medium by which light travels from one point to another. If it be similar to material media (such as air in the case of sound), it is to be expected that the velocity with which light travels with reference to a fixed observer will be affected by the movement of the ether relative to him. With so small a drift as even 31 miles per second, however, very little effect could be produced on the velocity of light, 186,000 miles per second; and only by very accurate means could it be detected. Perhaps the most obvious method of procedure, yet, nevertheless, only recently proposed, is that described by the Hon. R. J. Strutt¹ in November last. The proposal is to compare the velocity of light differentially with and against the drift at a time when it is horizontal. The method is an extension of Fizeau's well-known method of determining the velocity of light, and virtually amounts to measuring this velocity in the two directions. A difference of velocity of twice that of the drift is to be expected; and there appears to be no reason why this experiment, if practicable, should not at length settle the question of ether-drift. Unfortunately, the experimental difficulties to be overcome would be enormous. Apart from the very great one of securing equality of phase in the two rotating wheels which are essential to the determination, it is to be remembered that Fizeau's method of determining the velocity of light does not lend itself to great accuracy. In effect, this velocity would have to be measured correct to one part in ten thousand, and even the best of recent determinations can hardly claim such freedom from error.

But, although it has up to the present been impracticable to carry out an experiment of this type to the necessary order of exactness, viz. $\frac{u}{V}$, where u is the velocity of the supposed drift and V is the velocity of light, observations of a different type have been conducted, in which an accuracy beyond $\left(\frac{u}{V}\right)^2$ has been successfully attained. This means a measurement correct to less than one part in a hundred million. The first of these experiments—the famous one of Michelson and Morley²—is doubtless sufficiently familiar to make more than a brief description unnecessary. It is, in principle, to compare the

¹ R. J. Strutt, *New Quarterly*, November 1907.

² Michelson and Morley, *Phil. Mag.* vol. xxiv. 1887.

times of passage of light along equal distances, the one parallel and the other perpendicular to the drift of the ether. A difference of these times of the order of $\left(\frac{u}{V}\right)^2$ is to be expected; but, although this is so extremely small, the advantages which arise from the possibility of now making the light travelling by the different paths eventually recombine bring it within the limits of delicate measurement.

The actual method is the following: A beam of light is split into two parts by incidence on a plate of glass placed at 45° to its direction. Part is reflected at right angles to its original direction, and the remainder is transmitted with direction unchanged. By means of two mirrors placed at equal distances from the glass plate and normal to the respective beams, the latter can be returned to the same point, and, being in a condition to interfere regularly, will form a series of interference bands. The positions of these will depend upon the times taken by the light in traversing the two paths. If, from any cause, a relative change of time occurs, a shift of the bands is to be expected. Suppose now an ether-drift of magnitude u in the direction of the original undivided beam. The transmitted portion now travels toward the mirror with a velocity $V + u$, V being the velocity of light in stationary ether. After reflection, however, it will have a velocity (relative to the earth, of course) $V - u$. If D is the distance between the unsilvered glass and the mirror, the total time taken will be

$$\frac{D}{V + u} + \frac{D}{V - u}.$$

With regard to the other part of the beam, it might at first be supposed that the velocity of transmission perpendicular to the drift is unaffected, and that the time taken would be simply $\frac{2D}{V}$. This would lead to a difference of times equal to

$$\frac{D}{V + u} + \frac{D}{V - u} - \frac{2D}{V} = \frac{2D}{V} \cdot \frac{u^2}{V^2},$$

if powers of $\frac{u}{V}$ higher than the second be neglected. However, the velocity perpendicular to the drift is not unaffected by it; and when this effect is duly allowed for, the above difference is reduced by one-half. Now suppose that by rotating the whole apparatus, the ether-drift is caused to be along the path of the

other beam. The time difference will now be in the opposite sense, and therefore the total time difference introduced by turning through 90° will be $\frac{2D}{V} \cdot \frac{u^2}{V^2}$. In the later experiments of Michelson and Morley the shift of the interference bands to be expected was 0.4 of the width of a band. The extreme difficulty of carrying out an experiment of such delicacy can be imagined. It is obviously essential that the distance D should remain constant during the rotation of the apparatus, and such rotation is apt to call into play forces which produce a distortion sufficient to mask entirely the small expected effect. In ordinary work disturbances of this kind are of no consequence, but in experiments of this order of accuracy they become so serious that every precaution has to be taken to eliminate them. Eventually, the whole frame bearing the apparatus was floated on mercury, and could then be turned slowly by the application of very small forces. Many other difficulties had to be overcome; but at last spurious shifts of the interference bands were reduced to a minimum, and it was possible to state definitely that no movement amounting to 5 per cent. of the calculated value could be attributed to the supposed drift of the ether past the earth. The natural way of interpreting this negative result is to conclude that the ether in the near neighbourhood of the earth is in reality carried with it; but, as will be seen later, there are other means by which it could be accounted for. For the present, however, let us consider other and subsequent attempts to detect the supposed relative motion.

The following experiment is probably not so well known as the one just described, but it is extremely interesting because it depends upon a property of the ether other than that of conveying light. The method suggested is due to Prof. Trouton, and the work was carried out by him in collaboration with Mr. Noble¹ in 1903. The principle of the experiment is as follows: Imagine a charged electric condenser to be moving relatively to the ether in a direction parallel to its plates. A moving electric charge is equivalent to a particular current of electricity, and it is therefore to be expected to produce a magnetic field. When the condenser is moving as indicated, we have two equal opposite charges travelling in the same direction along parallel

¹ F. T. Trouton and H. R. Noble, *Phil. Trans.* vol. ccii. 1903.

planes separated by the thickness of the condenser. Owing to this separation, the magnetic effects of the two charges will not cancel one another, but there will be in the dielectric a resultant magnetic field perpendicular to the direction of motion and parallel to the plane of the condenser. If, however, the direction of motion be perpendicular to the latter plane, equal and opposite charges now travel along exactly the same paths; and, hence, are on the whole equivalent to a zero current. In the former case, therefore, there is associated with the condenser a certain amount of magnetic energy which is absent in the latter. On this account it was expected that such a condenser, freely suspended with its plane parallel to the ether-drift, would tend to turn through a right angle, so as to transform this surplus of energy; it could, however, be prevented from doing so by exerting a couple of the proper amount in the opposite sense. The detection of such a couple was the aim of the experiment, but, as in the work of Michelson and Morley, the quantity to be measured was of the order $\left(\frac{u}{V}\right)^2$, u and V having the same significance as before; and it was only after much work and many failures that definite information was obtained. To be exact, if N is the electrostatic energy of the condenser, the magnetic energy associated with it, when it is moving parallel to its plane with velocity u , is $N \cdot \left(\frac{u}{V}\right)$ and the couple tending to turn it, which attains a maximum value when the plane makes an angle of 45° with the direction of motion, is in the latter position also measured by $N \cdot \left(\frac{u}{V}\right)^2$.

With regard to the details of the actual experiment, the condenser, composed of successive sheets of mica and tinfoil, was suspended by a vertical phosphor-bronze strip, which served, not only as the means of controlling the movements of the condenser, but also for charging one set of plates. The other plates were earthed by means of a wire dipping into sulphuric acid, and a difference of potential of 2,000 volts was maintained by means of a Wimshurst machine. A mirror attached to the condenser viewed with a telescope and scale indicated its movements. Many precautions had to be taken to avoid disturbing influences, such as electrostatic attractions between the condenser and the walls of the enclosing case, air

draughts, and heating effects. These eventually having been practically eliminated, times were chosen when the supposed ether-drift was horizontal, and the plane of the condenser placed at 45° with its direction. It was then charged to a difference of potential of 2,000 volts, and a deflection looked for. The calculated value of the deflection, taking into consideration the earth's orbital motion only, was 3·4 centimetres, or, allowing also for the sun's proper motion, and experimenting in March at times when the resultant motion is horizontal, 6·8 centimetres. No deflection approaching either of these values was obtained, the largest barely exceeding 5 per cent. of the calculated value, and the authors considered the observations as being conclusive against the existence of any such effect as that sought.

Here, then, are two negative results which, on the face of them, point to the conclusion that the ether near the earth is dragged with it. But if this be admitted, difficulties at once arise in the ordinary explanation of aberration. Regarding the ether as behaving like a material medium, a velocity varying with the distance from the earth would produce an aberration of the stars differing for different latitudes. There is no record of a variation attributable to this cause having been observed, and, apparently, this cannot be due to lack of accuracy in measurement. The alteration of the angle of aberration produced by the motion of the earth on its axis has been detected, and, as it only amounts to about 2 per cent. of the whole at the equator, it is quite unlikely that the comparatively large variation which one would expect to be caused by a viscous ether should have been overlooked. Besides this consideration, there is the result of the experiment performed by Sir Oliver Lodge,¹ in which he aimed at detecting a drag on the ether near rapidly revolving steel discs. There is no space to fully describe the method of procedure here. Suffice it to say that two parts of a beam of light, after passing by opposite cycles round the interspace between two parallel discs, were caused to produce interference bands. If, now, the discs were rotated, and in doing so dragged the ether with them, the one part of the beam would be accelerated and the other retarded. This would produce a shift of the bands, but so little was observed that it was concluded that the velocity of light between the steel plates

¹ O. J. Lodge, "Aberration Problems," *Phil. Trans.* vol. clxxxiv. 1893.

moving together in their own plane an inch apart is not increased or diminished by so much as $\frac{1}{100}$ th part of their velocity. It is true that it is not justifiable, therefore, to assume that there is no viscous drag between the ether and the earth, for the mass of the earth and its velocity are extremely large in comparison with the corresponding quantities in the above experiment; but, combined with the previous consideration, the negative result may be regarded as a weighty argument in favour of a non-viscous ether. Accepting this position, it is necessary to seek for another explanation of the results of the attempts to measure ether-drift.

A suggestion due to Prof. Fitzgerald was that the forces between the molecules composing any body depend on the angle they make with the direction of motion through the ether, and therefore that the dimensions of the body vary with its position relative to the motion. If it be supposed that the length of any piece of material is shorter when parallel to the velocity through the ether than when perpendicular to it, and

that by the fraction $\frac{1}{2} \left(\frac{v}{V} \right)^2$ of the length, an alternative explanation of the negative results is obtained. In the case of the Michelson-Morley experiment such a change in dimensions due to rotation would alter the lengths of paths of the two beams in just such a way as to compensate the time difference due to the ether-drift. In the Trouton-Noble experiment, also, the effect would be to alter the electrostatic capacity of the condenser, so that the total quantities of energy associated with it would be equal in the two cases, thus eliminating the tendency to turn. In the hands of Professors Larmor and Lorentz¹ substantial theoretical reasons for such a change of dimensions have been adduced, and the theory has been extended to include the relation of physical quantities other than length to the direction of motion through the ether.

Into this theory it is not here proposed to enter, this paper being intended mainly as a discussion of actual experimental results. The question arises, "Is it not possible, if such a change of length really occurs, to devise investigations capable of detecting it?" It is true that the promoters of the complete theory claim that motion through the ether will so affect all the

¹ H. A. Lorentz, *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern*, Leiden, E. J. Brill, 1895.

physical quantities involved in any particular experiment as to produce, on the whole, no observable effect, and that, therefore, every conceivable attempt to measure ether-drift is predestined to failure. But it is to be remembered that it is unnecessary to assume any change other than that of length, in order to account completely for the two negative results previously referred to. There is, therefore, some justification for the attempts which have been made to discover whether the supposed alteration of dimensions does occur or not; although, in the light of their results, one is almost driven to the conclusion that its detection will really always be impossible. The experiments are, however, worth recording, if for no other reason, on account of their extreme delicacy. The quantity to be measured is again extremely small, being only a change of length of one part in two hundred million, it being the fraction $\frac{1}{2} \left(\frac{u}{V} \right)^2$ of the whole; for example, if we consider a particular diameter of the earth perpendicular to the direction of motion through the ether, it is supposed to be reduced by a few centimetres only when it becomes parallel to the ether-drift.

No direct attempt at measuring the change can succeed, because it is necessary that equal lengths of all materials suffer equal changes in length. The Michelson-Morley experiment was carried out using both wooden and stone frames; and the material used as the dielectric in the Trouton-Noble condenser was mica, while the plates were of tinfoil. It is obviously the natural conclusion, in view of the supposed relative alteration in size being equal for four such different substances, that all solid bodies, at any rate, are affected similarly. In the experiments to be described, quantities were measured which might reasonably be expected to vary with alteration in length.

The first of these investigations—that in which Lord Rayleigh¹ determined that the motion of the earth through the ether produces no sensible double refraction—was based on the following considerations: It is well known that if a piece of unstrained glass be placed between two crossed nicols, no light is thereby caused to pass through the analysing nicol; the application of stress to the glass, besides deforming it, causes it to become doubly refracting, thus producing a partial revival of the light through the analyser. Is it not, therefore, to be

¹ Lord Rayleigh, *Phil. Mag.* 6th Series, vol. iv, 1902.

expected that a deformation such as that supposed to be produced by motion through the ether will be accompanied by a corresponding stress in the material sufficient to cause sensible double refraction? If such double refraction exists, it should be possible, by placing a piece of transparent material between two nicols whose common axis is perpendicular to the ether drift, and setting the analyser for complete extinction, to observe a revival of the light on rotating the whole apparatus through 90° . As a matter of fact, in the actual experiment the calculated effect was too small to make this method possible, and the effect was tested for by comparison with known amounts of double refraction produced in a strip of glass by small stresses. Such a horizontal strip of glass loaded at the ends and supported in the middle, when placed between crossed nicols, produces revival of the light except in the neighbourhood of a horizontal band in the middle, called the "neutral axis," because there is no stress there. Just above and below this band, the strain is very slight indeed, and can be made of the order of that expected on account of motion through the ether by suitable adjustment of the stress. By including this apparatus, together with the transparent material under examination, between the two nicols, Lord Rayleigh was able to reduce the test to one for an apparent displacement of the neutral band. For the strain above the neutral band would be an extension, while that below would be a compression; the addition, therefore, of what was equivalent to a uniform strain over the whole piece would alter the resultant position of no strain. Observations of the behaviour of this band upon rotation were carried out with water, carbon-bisulphide and glass, in each case using considerable lengths of the substances so as to multiply the effect; in no case, however, was there a movement so large as that calculated. For the liquids, the experiment was of sufficient delicacy to conclude that not 2 per cent. of the calculated double refraction was observable. The experiments with glass, owing to increased difficulties such as loss of light, were by no means so conclusive as to a negative result. They were, however, repeated subsequently with improved apparatus by Prof. Brace,¹ with the result that it can be definitely stated that, in the case of glass also, no double refraction of the order expected can be detected.

¹ D. B. Brace, *Phil. Mag.* 6th Series, vol. vii, 1904.

A further attempt at measuring any deformation caused by motion through the ether was the result of a suggestion of Prof. Trouton some years ago. It is being carried out by him and the writer, but is now so near completion that, in all probability, the results will have been published by the time this article appears. It will, therefore, not be amiss to include a short description of it. The principle underlying the investigation is a very simple one, and in no way suggests the extraordinary difficulties attending the practical measurement. It is well known that the electrical resistance of a wire can be measured with great accuracy, and, provided such resistance depends upon the direction of ether-drift, its measurements should be a suitable means of testing the question. For simplicity, imagine a straight wire of resistance R , length l , and cross-sectional area A , occupying a position with its length perpendicular to the drift of the ether. If ρ be its specific resistance, we have—

$$R = \rho \frac{l}{A}$$

Now suppose the wire turned through a right angle. Upon the contraction hypothesis, the length will now be reduced by an amount $\frac{1}{2} l \left(\frac{u}{V}\right)^2$, or, writing β^2 for $\left(\frac{u}{V}\right)^2$, the length is reduced by $\frac{1}{2} l \beta^2$. One diameter of the wire will remain perpendicular to the drift, and will therefore be unaltered by the rotation; the other, originally parallel to the drift, will now be perpendicular to it and therefore an increase in the ratio $(1 + \frac{1}{2} \beta^2) : 1$ is to be expected. This will increase the area A by an amount $\frac{1}{2} A \beta^2$. Now if the specific resistance of the wire does not depend on the motion through the ether, the new resistance (R^1) of the wire will be

$$R^1 = \rho \frac{l(1 - \frac{1}{2} \beta^2)}{A(1 + \frac{1}{2} \beta^2)} = R(1 - \beta^2),$$

neglecting powers of β higher than the second. That is to say, the resistance of the wire parallel to the drift is to be expected to be less than when perpendicular in the ratio $(1 - \beta^2) : 1$. If, then, so small a difference of resistance can be detected, a means is thus afforded of testing for the Fitzgerald-Lorentz shrinkage, and, consequently, for the magnitude and direction of ether-drift. For this investigation, the method in use is the Wheatstone bridge method for the comparison of resistances. Four coils of

wire are wound upon flat rectangular frames. They are arranged upon a stand one above the other, so that in two of them the wire is perpendicular to that in the remaining two; and each pair composes two opposite branches of the Wheatstone bridge arrangement. The battery and galvanometer are put in in the usual places; and adjustment for no current through the galvanometer is secured by means of a slider which connects, through it, the junctions of adjacent arms.

The intended method of procedure, which it has been necessary, however, to modify in actual practice, is as follows: Let the stand be placed, at the time when the ether drift is horizontal, so that two coils are parallel and two perpendicular to it, and adjust the slider so that there is zero current in the galvanometer. Now rotate the stand through a right angle, so that the wire which was parallel is perpendicular. If any change of resistance is consequent upon this rotation, the balance will be destroyed, and a current will flow. It can be easily shown that a change of resistance equal to that previously calculated would, neglecting the resistance of the battery, produce a current in the galvanometer of magnitude $\frac{E\beta^2}{R+g}$, where E is the electromotive force of the battery, g the resistance of the galvanometer, and R the resistance of each coil. In the present apparatus R is 11 ohms and g 10 ohms; the battery used is a single storage cell, so that E is about 2.1 volts. The current to be expected, therefore, at a time when $\beta = 10^{-4}$, is about 10^{-6} ampere. The sensitiveness of the galvanometer is such as to give almost exactly 1 centimetre scale deflection with this current; and, provided that disturbing causes are removed, it becomes quite easy to say whether such a current exists or not. But it is an extremely difficult thing to eliminate these disturbances sufficiently to make a determination possible. Their nature and the means of getting rid of them will be described in detail in the proper place; here, it will suffice to mention only one or two of them. One of the greatest difficulties is the changes of resistance which occur owing to unequal heating of the various wires. This is, no doubt, due in part to draughts of air, and in part to the currents which the wires are conveying. In the earlier forms of the apparatus, when bare wire was used and the stand itself was rotated inside an enclosure, the variation of the current through the galvano-

meter was so rapid that it was impossible to observe the effects of rotation. Even now, when the wire is uniform throughout and thickly covered with gutta-percha, and the stand and enclosure are rotated bodily, variation of the current is not absent. It is, however, sufficiently slow to admit of an effect of the calculated amount being observed. No attempt is made to reduce the galvanometer current actually to zero; the effect looked for is a change of the current then flowing upon rotation of the apparatus. Another serious disturbance—and one which at one time led us to suspect a positive result—is that due to changes of resistance caused by stresses in the wires. At that time the apparatus was fitted up in such a way that the slider and balancing bridge did not move when the coils were rotated, with the result that parts of the wires whose resistances were being compared became twisted upon rotation, balance being thus destroyed. This effect has now been removed by turning the apparatus bodily, with the exception of the galvanometer; and the whole strain now occurs in the wires leading to the latter. It is now possible to say definitely that the motion of the earth through the ether does not produce a change of resistance of as much as 5 per cent. of the calculated value. The result of the experiment, therefore, like those previously described, fails to give any indication of the existence or non-existence of ether-drift. All that may be stated is that, if any alteration of dimensions of the wires does occur, the specific resistance of the material forming it changes also in a compensatory fashion.

All the above experiments have been devised for the measurement of quantities of the second order, *i.e.* quantities of order of magnitude $\left(\frac{u}{V}\right)^2$. There is one experiment, however, which has been carried out by Lord Rayleigh,¹ in which a first order effect might be expected. Like the others, its result is negative; and it is to be noticed that the Fitzgerald-Lorentz shrinkage theory will not account for this. It is well known that if a beam of plane polarised light be incident upon a plate of crystalline quartz, and pass through it parallel to its optic axis, the light, on emerging, is still plane polarised; but the plane of polarisation is rotated through an angle depending upon the thickness of the quartz plate used. It is usual to account for

¹ Lord Rayleigh, *Phil. Mag.* 6th Series, vol. iv. 1902.

this rotatory polarisation by supposing that the incident light is split up by the quartz into two circularly polarised components which travel through it with different velocities; upon emergence these components recombine, forming again plane polarised light, but owing to the difference of phase introduced by the unequal velocities of propagation, the emergent light is polarised in a plane making an angle with that of the incident light. This angle is directly proportional to the difference of phase caused by the passage through the quartz. If, for any reason, the phase difference changes, an alteration in the amount of rotation will be the result. Suppose, for instance, the velocities of propagation of the two circularly polarised components to be increased by the same amount. Their difference will be the same as before; but the phase difference introduced in traversing a constant length of quartz will be reduced by this uniform increase of velocity. On the other hand, if both velocities are reduced by equal amounts, the phase difference is increased in the same length of quartz. Such equal changes of velocity may be expected to result from a drift of the ether in the quartz along its axis; and, on this assumption, there should be a difference of rotatory polarisation according to whether the ether-drift is in the direction of the beam or directly opposed to it. It is true that there is reason to suppose that, in material substances, the velocity of ethereal motion differs from that in vacuo; but for ordinary materials—as will be shown later—the two are quite of the same order of magnitude. Lord Rayleigh was able, by means of carefully observing the rotation of the plane of polarisation produced by five thick quartz crystals in series between two nicols, to show that its amount was independent of the supposed direction of ether-drift.

Before concluding this review of experiments on ether-drift, it will be well to devote a paragraph to what is known with regard to the internal effects of moving matter. If the existence of an ether having the properties ascribed to it be admitted, it cannot be doubted that those parts of it, which actually occupy the same space as material substance, are considerably modified by the presence of the latter. For instance, the ether-waves which constitute light travel more slowly in air than in vacuo, and more slowly still in such media as water and glass. Material substances, although not themselves conveying the luminous vibrations, have the power of reducing the rate at which they

travel. Whatever the modification of the ether produced by the presence of matter may be, it is a natural question to ask, "When a piece of material moves, does it carry with it the whole of the particular part of the ether which it contains; or does it merely remove the modification to the various portions of the ether through which it passes?" Of these two alternatives, the former seems to be the less likely. It is somewhat difficult to believe that an abrupt change in the velocity of the ether occurs at the boundary of moving matter. Lodge's experiment shows that, just outside pieces of material of ordinary size, there is no sensible drag on the ether; while the above supposition leads to the conclusion that, just inside, the ether has the full velocity of a moving body. Moreover, the second supposition has considerable experimental support. As has been shown by Fresnel, it is convenient to regard the modification produced in the ether by the presence of matter as an effective change in density; or, the ether is conceived as being loaded by the matter and, hence, incapable of conveying luminous vibrations so rapidly as in vacuo. If we denote by ρ the density of the unmodified ether, and by ρ' the density inside a moving body, the latter is continually changing from ρ to ρ' the density of the different portions of the ether which it from time to time includes. The effect of this is equivalent to the carrying forward of an additional ethereal density of magnitude $\rho' - \rho$ with the full velocity with which the body is travelling; this is again equivalent to saying that the whole of the ether within the body is dragged forward by the motion of the latter with a velocity less than the full velocity in the ratio $\frac{\rho' - \rho}{\rho}$. On Fresnel's assumption that the density of the ether in any material is inversely proportional to the velocity of light in that material, we obtain the following relation:

$$u' = \left(1 - \frac{\rho}{\rho'}\right)u = \left(1 - \frac{V'^2}{V^2}\right)u = \left(1 - \frac{1}{n^2}\right)u, \quad .$$

where u is the velocity of the body, u' the effective velocity with which the ether in the body is dragged forward, V and V' the velocities of light in vacuo and in the body respectively, and n the refractive index of the material forming the body. It is probable that this partial drag of the ether does not occur, and that what really occurs is a successive modification of the different parts of the ether; but the effect on the motion of

light in a moving body is, nevertheless, the same as that which a partially moving ether would be expected to produce. This has been shown by the direct experiment of Fizeau¹ on the effect of moving water on the velocity of light. The experiment has later been repeated and confirmed by Michelson and Morley.² It consisted in causing to interfere two beams of light, one of which travelled with, and the other against, running water. The position of the interference bands was found to be different according to whether the water was moving or not. The shift was such as to indicate that the light travelling with the water was accelerated, and the other beam retarded, not by the full velocity of the water, but by an amount less in the proportion just calculated; and the effect observed was exactly equivalent to that which would be produced by a partial drag of the ether by the moving water.

It is noteworthy, too, that a supposition of this kind is necessary to account for the observation of Airy and Hoek that the angle of stellar aberration is independent of the material filling the telescope. On the assumption that the ether moves freely through a water-filled telescope, one would expect that the observed aberration would be greater than when it is filled with air, because light travels more slowly in water than in air. The aberration is found, however, to be the same in both cases; and this is exactly accounted for by postulating the partial drag previously referred to.

Little more remains to be said. This paper is intended merely as a review of the experimental work on ether-drift. Much has been written with a view to explaining this series of negative results. The supposition that the ether in the neighbourhood of large bodies like the earth is carried wholly with them at once introduces difficulties with regard to stellar aberration. Moreover, it is surely improbable that, while the ether inside moving matter appears to be only partially dragged with it, the ether just outside should appear to be dragged forward with the full velocity. On the other hand, supposing the ether near the earth to be free, explanations of the negative results can be formulated by conceiving that physical quantities in general are dependent upon the relative motion of the observer and the ether, and that in such a manner as to make

¹ Fizeau, *Ann. de Chimie et de Physique*, 3rd Series, tom. xvii. 1859.

² Michelson and Morley, *American Journal of Science* (3), vol. xxxi. 1886.

always impossible terrestrial detection of ether-drift. There is another alternative which, after all, may be the correct one, namely, that it is not justifiable, particularly as so little is known about the ether, to attribute to it all the properties of a material medium, and expect it to behave precisely as we know the latter would do under similar circumstances. Nothing can, however, be stated with certainty, and the problem must be regarded as not yet solved. The experiments described, with the exception of Fizeau's, have not disclosed the information expected and desired; but they are surely interesting, if for no other reason, because they show to what extreme accuracy measurements can now be pushed when circumstances require it.

ANIMAL PARTHENOGENESIS

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Or the numerous problems which are at the present time awaiting solution at the hands of biologists, none are more fundamental than those connected with sexual reproduction. The questions which cluster around this subject touch almost all departments of biology; they include heredity, the origin of variation, the causes of cell-division, the function of the chromosomes, centrosomes, and other nuclear or cell structures, as well as the function and origin of sex, and probably the actual physical or chemical forces upon which life itself depends.

One method of attempting the solution of these problems consists in comparing cases of normal sexual reproduction with those in which it is partially or wholly suppressed, that is to say, in comparing the processes of reproduction consequent on fertilisation with those observed when conjugation of male and female elements does not take place. Such cases come under the general description of parthenogenesis, which may be defined as reproduction by means of a germ-cell which has not undergone conjugation.

Cases of parthenogenesis may be classified in various ways; we may first divide them into those which occur naturally as part of the normal life-history of the species, and those which occur only under artificial stimuli (artificial parthenogenesis), but it is not certain whether there is any really natural line of distinction between the two. Then we find that parthenogenesis graduates naturally into such forms of asexual reproduction as budding, which, however, are not as a rule included under the term, although it is difficult to define one without including the other. For example, it is not easy to say that the production of internal buds in the trematodes, sponges, or Polyzoa differs fundamentally from reproduction by unfertilised eggs in an Aphid, or from the "pædogenesis" of some of the Cecidomyids. In this latter case genital cells are produced in the larva of a dipterous fly, which develop and

reproduce the species, but if the process took place rather earlier in the life-history, before the embryonic period had closed, we should class it as budding, or call it embryonic fission. This does happen in some embryos of parasitic Hymenoptera, as has been shown by Marchal¹ and Silvestri,² and it does not seem to differ fundamentally from the pædogenesis of *Cecidomyia*. So that, although some cases of alternation of generations are commonly described as consisting in reproduction alternately by budding and by a sexual process, and others by alternation of parthenogenesis and bisexual reproduction, yet it is not clear that any true distinction can be drawn between them.

The most frequent form of natural parthenogenesis is that which alternates with a bisexual process, and in this class we get every stage between cases where the parthenogenetic and bisexual generations do not differ in any character except their manner of reproduction, and those in which two or more wholly different generations are produced in the life-cycle.

In the rotifer *Hydatina*, for example, it seems that the difference between the females which will produce eggs that must be fertilised, and those which reproduce parthenogenetically, depends on whether the young female has copulated with a male or not.³ In one case she produces large eggs with much yolk which must be fertilised, and after a resting period develop into females, and in the other she produces much smaller eggs which are not fertilised and yield males. Another class of female, not differing in appearance, produces eggs of an intermediate type, which are not fertilised, and produce females.

In the Daphniidæ also there is no morphological difference between the bisexual and parthenogenetic generations; in fact, the same female may produce parthenogenetically either females, males, or both, and may at another time lay eggs requiring fertilisation.

The number of parthenogenetic generations which normally intervene between one conjugation and the next varies largely in different species of Daphniidæ, but there is considerable evidence⁴ to show that the kind of egg produced by some

¹ *Arch. Zoo. Exp. et Gen.* (4) ii. p. 257.

² *Ann. Scuola Agric. Portici*, vi. 1906, p. 1.

³ Lensen, *La Cellule*, xiv. 1898, p. 421.

⁴ Issakowitsch, *Biol. Centralbl.* xxv. 1905, p. 529.

females at least depends largely on the nutrition. Somewhat similar statements have also been made about *Hydatina*,¹ but the latest work² on it indicates that they are mistaken, so perhaps judgment must be suspended about the Daphniidae also.

In the Aphididae the conditions resemble those in the Daphniidae, but are more complicated. The bisexual generation generally differs considerably from the parthenogenetic, and not infrequently there are two or more parthenogenetic forms which differ widely among themselves. Here again the number of generations which intervene between one fertilised egg and the next varies in different species, and while in some it is said that the production of a bisexual generation depends on the conditions, in others³ it is maintained that always the same number of parthenogenetic generations occurs, whatever are the conditions under which the animals live. Again, in other species, the same individual female may produce both parthenogenetic and fertilisable eggs, and among the former there may be both males and females, or only one or the other. In *Aphis* and its allies, therefore, we get a very great variety of conditions, ranging from simplicity of life-history very like that of *Daphnia* to complexity hardly surpassed in the life-cycles of the trematodes.

As an example of these complicated life-cycles, one may take the genus *Chermes*.⁴ *C. abietis* lays fertilised eggs on the fir *Picea* (*Abies*), which develop into wingless parthenogenetic females. These hibernate, and lay eggs producing winged parthenogenetic individuals, which migrate to the larch, and give rise to a wingless parthenogenetic generation. These, after hibernation, lay eggs, some of which develop into wingless forms, which continue to reproduce children like themselves; the rest develop wings, return to the *Picea*, and their parthenogenetic eggs develop into sexual males and females like those with which the cycle started two years before. There are, therefore, in the life-history of this species five different kinds of individuals, some existing only on the fir

¹ Maupas, *C.R.* cxi. 1890, pp. 310, 505.

² Punnett, *Proc. Roy. Soc. B*, 78, 1906, p. 223.

³ Tannreuther, *Zool. Jahrb.* xxiv. p. 609.

⁴ Cholodkovski, *Biol. Centralbl.* xx. 1900, p. 265. Börner (*Zoo. Anz.* xxxii. 1907, p. 413) finds that the life history is more complex than Cholodovski's description, and re-establishes the "parallel series" as originally described by Dreyfus (cf. *Cambr. Nat. Hist.* vol. vi. p. 585).

and others on the larch, and all arising parthenogenetically from individuals derived from a fertilised egg. Even more complicated instances probably exist, for it is said that in one species of *Phylloxera* twenty-one distinct forms occur between one sexual generation and the next.

In these cases which have just been described, we find a more or less regular cycle of alternation between bisexual and parthenogenetic generations; in some cases the number of the latter appears to be variable, and to some extent dependent upon environment, in others it is fixed and regular. Perhaps the best cases of regular alternation are provided by the gallflies (Cynipidæ), which have two generations in the year, one of which consists wholly of females and reproduces parthenogenetically, the other contains both sexes, and lays eggs requiring fertilisation. These flies lay their eggs in the leaves or buds of trees (generally oak), and as the egg develops, a gall grows round it, and is used as nourishment by the larva. Commonly the parthenogenetic generation appears in the season when the trees are without leaves, and the eggs are laid in buds; from the galls thus produced a bisexual generation arises in the summer, the females of which lay fertilised eggs in the leaves. Not only are the galls produced by the two generations very different, as might be expected from their time and mode of origin, but the flies themselves differ so widely that they were commonly placed in separate genera before the relation between them was known. The fly which has to lay its eggs deep down in the buds of an oak has an ovipositor some ten times the length of its daughter's, which has only to prick a soft leaf, and many other characters show a correlated difference.

It is interesting that a number of species of gallfly seem to have dropped the sexual generation entirely; they now produce only one generation in the year, which consists entirely of females, and these, as far as is known, go on reproducing by parthenogenesis year after year indefinitely without a male ever appearing.

Among the sawflies (Tenthredinidæ) the conditions are somewhat different. In them, in some cases, we appear to get a mixture of parthenogenetic and fertilisable eggs produced by the same female, and in these it is usual, but not universal, that the parthenogenetic eggs should give origin to males. But in other species males are excessively rare or unknown, and as in

the case of the purely parthenogenetic gallflies, they appear to be able to reproduce for an indefinite number of generations without the intervention of a true sexual process. On the other hand, a very large proportion of sawflies are always bisexual, and their eggs never develop unfertilised. A disappearance of the male and indefinite reproduction by parthenogenesis appears to have occurred also in some species of *Ostracoda* among the Crustacea, but it is very exceptional in the animal kingdom.

The case of the sawflies, which produce males from unfertilised, females from fertilised eggs, leads on to that of the honey-bee and others of the same class, in which all the eggs appear to be alike, but when they are fertilised they give rise to females, when they develop without fertilisation they yield males. It appears to be established almost beyond possibility of doubt that in many of the aculeate Hymenoptera, and also some of the parasitic families, the sex is determined by fertilisation. And in some of the cases mentioned above, *e.g.* *Daphnia*, *Aphis*, and *Hydatina*, although parthenogenetic eggs might give rise to either males or females, yet, as in the bee, fertilised eggs are all female, so that here again it is possible that fertilisation determines the sex. But in the one case (the bee, etc.) the eggs are all alike, and differ only in whether they are fertilised or not, in the other some eggs require fertilisation if they are to develop, others are not capable of it in any case.

Artificial differs from natural parthenogenesis in the fact that it does not occur without some external stimulus which the eggs will not commonly experience in nature. But in some species, *e.g.* certain moths, parthenogenesis occurs exceptionally in nature, but can apparently be made more frequent by applying certain stimuli, such as shaking the eggs or treating them for a very short time with acid. Also in the Echinoderms, the group in which artificial parthogenesis has been most thoroughly studied, some species are naturally parthenogenetic, and it would seem that the stimulus supplied brings into action a tendency which normally is not strong enough to take effect.

The artificial means used for causing eggs to develop without fertilisation are very varied; it was first done by Loeb, by the addition of magnesium salts to the water containing them, and he supposed the magnesium exerted a specific influence. It soon, however, appeared that other salts were effective, and ionisation and osmotic pressure were suggested as causes. Since then

similar effects have been brought about by change of temperature, and in the case of starfish eggs by anæsthetisation with carbonic acid during the maturation process.¹ It is possible, therefore, that the stimulus applied may be of various kinds; and what is required is to bring into activity a tendency to cell-division and development which is not normally aroused except by the entrance of the spermatozoon.

Loeb, however,² considers that by far the most important factor, and in most cases the only one, is the extraction of water from the egg by osmotic tension. The eggs of some marine worms have been caused to develop to larvæ by the addition of calcium or potassium ions without raising the osmotic tension; but for probably all other cases he regards the extraction of water as the exciting cause. That it is not the subsequent entrance of water when the eggs are put back into their normal surroundings is shown by the fact that the eggs of some sea-urchins develop in solutions of higher osmotic tension without any return to normal conditions, and also because dilution of the sea-water does not bring about development.

These facts lead Loeb to an interesting comparison between artificial fertilisation by hypertonic solutions, and normal fertilisation by a spermatozoon. In echinoid eggs, one of the most characteristic phenomena of fertilisation is the almost instantaneous formation of a membrane around the egg when the spermatozoon enters. Loeb shows that this is due to expulsion of water from the egg, since if the process is watched in eggs which have lain for many hours in sea-water, so that it takes place slowly instead of rapidly, vesicles are seen to form round the egg as the water is expelled, and these coalesce to form the membrane. Unfertilised eggs may be induced to form a similar membrane by putting them in rather strong solutions of salts or sugar; but such eggs generally break up rapidly owing to the high osmotic tension. It seems, therefore, that both in fertilised eggs and in those made to develop parthenogenetically by artificial means, there is loss of water, and it may be that this provides the stimulus which causes the egg to segment.

On the other hand, Kellogg³ finds in the eggs of the silk-

¹ Delage, *C.R.* cxxxv. Nos. 15, 16, 1902.

² *Pflüger's Arch. f. ges. Phys.* vol. ciii. 1904, p. 257.

³ *Biol. Bull.* xiv. 1907, p. 15.

moth that although a large variety of stimuli lead to an increased proportion of eggs which segment, yet there is no common characteristic such as dehydration which can be regarded as a general cause; and since Loeb admits that the eggs of some animals may be caused to develop by other means—though abnormally—it is perhaps premature to regard loss of water as the invariable stimulus required for their development.

Hitherto we have considered only what may be described as the natural history of parthenogenesis, the conditions under which it is found in various groups, and how it is related to bisexual reproduction. But one of the chief interests of a comparison of these two forms of reproduction is in the differences in the maturation of the eggs in the two cases. In eggs which are to be fertilised it is invariably found that the chromosome number is reduced to one-half that which is normal in the somatic cells of the species, and this almost always happens by a preliminary pairing of the chromosomes (or some equivalent process) followed by two nuclear divisions, one of which separates the members of each pair into different daughter nuclei, and the other splits each into two similar halves. These two divisions give rise to the first and second polar bodies, which, with very rare exceptions, are formed by eggs requiring fertilisation. A similar process occurs in the maturation of the spermatozoa, a single unreduced cell giving rise by two successive divisions to four cells, each containing half the normal number of chromosomes.

—The actual process of "chromosome reduction" appears to vary somewhat in different cases, and there has been much disagreement among authorities as to how it typically takes place. Perhaps we may regard the phenomena described by Farmer and Moore¹ as most generally typical. At the beginning of the "meiotic phase" (the stage during which the reduction divisions take place), chromatic loops appear in the nucleus, in number half as many as the normal number of chromosomes in ordinary cells. These loops frequently show signs of a longitudinal split. They become shorter and thicker, and contract into characteristic ring or loop shapes which arrange themselves on the spindle in such a way as to be divided transversely instead of longitudinally. Even at this stage the

¹ Farmer and Moore, *Q.J.M.S.* vol. xlviii. 1905, p. 489.

longitudinal split which was visible earlier may still be recognised, and at the next division, which follows rapidly on the first, the transverse halves of the original chromatic loops are divided longitudinally along this split. The essentials of the process are therefore (1) that half the normal number of chromosomes appear in the nucleus; (2) that they are divided by successive transverse (heterotype) and longitudinal (homotype) divisions into four cells; and (3) since the original loops almost certainly consist each of two chromosomes paired end to end, which are separated by the transverse division, each of the four resulting nuclei contains half the normal number of simple chromosomes.

In animals the reduced cells are normally converted direct into germ-cells; but in plants a whole generation with reduced nuclei may alternate with that derived from a fertilised egg-cell. In recent years it has been shown¹ that in malignant growths of various sorts nuclear divisions of the "meiotic" type occur, with a reduced number of chromosomes, and it seems not impossible that this fact is connected with the parasitic nature of the growth. The developing germ-cells of animals and frequently the post-meiotic generation in plants behave more or less as parasites having an independent existence at the expense of the rest of the body; and it is therefore perhaps not mere coincidence that similar nuclear structures and modes of division should be found in both gametogenic cells and in malignant growths.

If this process of chromosome reduction took place in eggs which were not fertilised, and if there were no means of restoring the chromosomes to the normal number during development, it is evident that in every parthenogenetic generation the chromosome number would be halved, a process which could not continue very long. Hence it is found that the maturation of parthenogenetic eggs differs, in most cases at least, from those which must be fertilised, and where this difference does not occur some unusual process is found elsewhere in the development of the germ-cells.

The most frequent type of maturation in parthenogenetic eggs is that only one polar body is formed and there is no reduction in chromosome number. No pairing of chromosomes takes

¹ Farmer, Moore, and Walker, *Proc. Roy. Soc.* vol. lxxii. 1903, p. 499; also 1904, 1906.

place beforehand, and the single nuclear division consists in a longitudinal splitting of each chromosome, and a separation of the halves into different nuclei. This is found in parthenogenetic eggs of the Daphniidæ¹ and Ostracoda² among the Crustacea, and the Aphididæ³ among insects, which include the majority of parthenogenetic species if cases which are commonly classed as budding are excluded. In this group there is no visible difference in the maturation of the eggs, whatever kind of individual those eggs are destined to produce; males and sexual and parthenogenetic females all seem to have identical maturation processes.

In the rotifer *Asplanchna*, on the other hand,⁴ it is stated that eggs which will develop into females have only one polar body, but those which yield males have two. A somewhat similar case is that of *Hydatina*,⁵ in which female parthenogenetic eggs begin a polar mitosis which is never completed; but the male and fertilisable female eggs have one complete polar division.

But quite a large number of parthenogenetic eggs have two maturation divisions and produce two polar nuclei, or three if the first one divides, as is frequently the case. This is so in all the Hymenoptera which have been examined, viz. the bees, wasps, sawflies, gallflies, and some belonging to the parasitic families. Two polar divisions are also recorded for the stick insect *Bacillus*.⁶ In *Artemia*⁷ (Crustacea) and in one of the Stylopidæ⁸ (insects) the formation of two polar bodies has been recorded; but it is stated that the second may return into the egg and conjugate with the egg nucleus, so apparently taking the part of a spermatozoon. In *Artemia*, however, this is not invariable, for often only one polar division takes place.

But in the other cases in which two polar divisions take place in a parthenogenetic egg, it is of interest to know whether reduction of chromosomes takes place or not. In some species, e.g. of gallflies and sawflies, the species may be reproduced for many generations without the intervention of a male, and if the

¹ Weismann and Ischikawa, *Ber. Nat. Ges. Freiburg*, iii. 1887.

² Woltereck, *Zeit. Wiss. Zoo.* lxiv. 1898.

³ Stevens, Carnegie Inst. No. 51, 1906.

⁴ Erlanger and Lauterborn, *Zoo. Ans.* xx. 1897, p. 452.

⁵ Lensen, *La Cellule*, xiv. 1898, p. 421.

⁶ Von Bahr, *Zool. Jahrb. Anat.* xxiv. p. 175.

⁷ Brauer, *Arch. Mikr. Anat.* vol. xliii. 1893.

⁸ Brues, *Zool. Jahrb. Anat.* xviii. p. 241.

chromosomes are halved at each generation it is evident that they must be doubled again somewhere during development. In these cases where females result from parthenogenetic eggs, the few observers¹ who have examined them agree in saying that no reduction takes place; there are two maturation divisions, but in each the chromosomes split and their number is not reduced. But there are other examples, of which the honey-bee is typical, in which it seems that the egg may develop without fertilisation, or may receive a spermatozoon and be fertilised. Here there can be little doubt that normal reduction takes place, and either the animal resulting from the unfertilised egg must have half the normal number of chromosomes in each of its nuclei, or a doubling must take place somewhere. It has been said² that in the bee such a doubling takes place, but more recent work makes it probable that the statement is mistaken. For the unfertilised egg of the bee yields a male, and in the spermatogenesis of the drone one of the maturation divisions is suppressed,³ a fact which suggests that only half the normal number of chromosomes is present. Also the number of chromosomes at the end of the maturation is sixteen, while the observer who stated that doubling took place in the unfertilised egg made the reduced number (before doubling) eight. Probably, therefore, he miscounted the number in the polar mitoses, and supposed that doubling had occurred when he found sixteen in the later divisions.

A similar disagreement has arisen as to whether the chromosome number is restored to the normal (*i.e.* doubled) in cases of artificial parthenogenesis. Most of such cases occur in eggs which have undergone reduction, and the majority of observers⁴ agree that the nuclei in the larvæ produced still have the reduced number. Others,⁵ however, have maintained that the normal number is restored. That restoration does not always or even usually happen seems fairly clear, but instances have been described⁶ where a nuclear division has begun, and pro-

¹ Henking, *Zeit. Wiss. Zoo.* vol. liv. p. 147; Doncaster, *Q.J.M.S.* xlix. 1906, p. 561.

² Petrunkevitch, *Zool. Jahrb. Anat.* xiv. 1901, p. 573.

³ Meves, *Arch. Mikr. Anat.* vol. lxx. 1907, p. 414.

⁴ *E.g.* Wilson, *Arch. Entwickl. Mech.* xii. p. 529.

⁵ Delage, *Arch. Zoo. Exp. et Gen.* (3) ix. 1901, p. 301.

⁶ Petrunkevitch, *Weismann Festschrift Zool. Jahrb.* 1904, p. 77; Kostanecki, *Arch. Mikr. Anat.* lxiv. 1904, p. 1.

gressed as far as the splitting of the chromosomes, and then the spindle has broken down and the nucleus returned to the resting condition. At the next division after such a process the chromosome number would of course be found to be doubled, and it is not unlikely that it is from the observation of cases such as this that the statements about the restoration of the normal number have been made.

These facts lead up to one very interesting conclusion, which could not have been reached apart from the study of parthenogenesis, namely, that half the normal number of chromosomes is sufficient for the life and growth of the individual. It has been supposed that in an individual derived from a fertilised egg each nucleus contains a double set of chromosomes, one derived from the egg and the other from the spermatozoon, but that if one complete set is present it will contain all the qualities necessary for the production of an individual. But Boveri¹ has shown that if the complete set is not present, even when the nucleus includes more than the necessary number of chromosomes, the egg will not produce a complete individual. This, as he points out, is very strong evidence in favour of the individuality and difference of function of different chromosomes.

Another cell-organ on which parthenogenesis sheds some light is the centrosome. In fertilised eggs it appears to arise from or in connection with the spermatozoon, but as it exists in the cells derived from virgin eggs it is clear that it can arise *de novo* and that the spermatozoon is not required for its production.

Probably the two most interesting subjects on which the study of parthenogenesis may shed light are those of variation and the determination of sex.

Weismann supposed that variation was largely caused by the conjugation of somewhat unlike nuclei in fertilisation, and this hypothesis has led to statements that parthenogenetic animals do not vary, or do so less than those which are sexually produced. Measurements of such forms as *Daphnia* and *Aphis*² have not confirmed this idea, but show that variability is not widely different in parthenogenetic and sexual species. And that such a conclusion was to be expected might have been inferred from the polymorphism of partheno-

¹ *Ergebnisse über die Konstitution, etc., des Zellkerns*, 1904 (Fischer).

² Warren, *Biometrika*, 1, 1902, p. 129.

genetic generations found, for example, in the aphids. These present a somewhat puzzling problem. It is frequently assumed that differentiation takes place at the reduction division during the maturation of the germ-cells; for example, the segregation of characters in a Mendelian hybrid (heterozygote) is commonly regarded as taking place at this stage. But in *Chermes* or *Phylloxera* a differentiation is found among the offspring of a single parent quite as striking as that in Mendelian segregation, and yet no reducing nuclear division takes place. The two cases are of course not quite comparable, for the Mendelian segregation separates characters (allelomorphs) permanently, and in the aphid certain characters become latent, and reappear in later generations. But clearly the cause of these differences between children of one parent must in this case be sought in something other than chromosome reduction.

The problems of sex are intimately connected with parthenogenesis, but although in some cases we seem to approach a solution, others offer little help, or may even appear contradictory.

In the bee and a number of other Hymenoptera sex appears to be determined by fertilisation. Virgin eggs yield males, fertilised eggs females. Hence it has been assumed by some that the spermatozoon bears the sex-determinant. But other parthenogenetic species produce both sexes from unfertilised eggs—for example, gallflies, aphids, and daphnids. In a few cases the parthenogenetic eggs which will yield females produce one polar body, those giving males two; hence it was suggested¹ that the female determinant might be separated from the eggs at the second polar division. But, again, in the aphids and daphnids eggs producing both sexes have only one polar division, and among the Hymenoptera females arise in many species from eggs which have two.

But one conclusion on this matter can be made with some confidence from the study of parthenogenesis, viz. that the sex is determined from the beginning of development, and is not as a rule altered by circumstances affecting the animal afterwards. A bee-larva may be made into a queen or a worker by circumstances, if it is female to begin with, but if it arises from an unfertilised egg no treatment will turn it from a

¹ Castle, *Bull. Mus. Harvard*, xl. 1903, No. 4.

drone to a female. The same conclusion is reached from the study of all kinds of parthenogenetic animals. It is conceivable, where a creature contains the determinants of both sexes, as appears to be the case in most animals produced from fertilised eggs, that circumstances may determine which sex dominates, but there is no real evidence for this, and many things make it improbable.

Parthenogenesis might be expected to help in answering the question "What is the function of sex?" but the result of such inquiry is disappointing. Some sort of sexual process is so widely distributed that it is often assumed to be universal. Yet several species of animals, both Insects and Crustacea, are known in which no male has ever been found, and which can live and reproduce to all appearance indefinitely without the occurrence of conjugation. It is true that these things have been studied for only a short time and by few observers, and that where males were once thought to be absent they have since been discovered, but when twelve thousand individuals of a species are reared and no male discovered among them, as has been done with *Cynips kollari*,¹ one may assume that bisexual reproduction must at least be so rare in the species as to be practically negligible. And yet the purely parthenogenetic species seem to persist and flourish not less vigorously than those which conjugate with unfailing regularity. The physiological basis of sex is still a mystery.

¹ *Alternating Generations*, Adler and Straton, Oxford, 1894, p. 165.

TUBERCULOSIS AMONG ANIMALS

By W. A. BREND, M.A., M.B., B.Sc.

THE splendid work done by the Royal Commission on Human and Bovine Tuberculosis, and the immense mass of observations recorded in the six large volumes of the report and appendix, afford material for research to independent investigators in directions other than those taken by the Committee. One of these is the problem of the curious and marked differences in the degrees of susceptibility to tuberculosis exhibited by the different classes of animals which were the subject of experiment.

The Commission was appointed to determine whether tuberculosis in men and animals is one and the same disease, and if, and under what conditions, transmission of it from animals to men takes place. For the purposes of the inquiry numerous experiments were made on dogs, cats, rabbits, rats, mice, goats, pigs, bovines, monkeys, apes, and guinea-pigs. Some of these animals were found to display remarkable powers of resistance to the disease, others were susceptible to extremely small doses of the virus. But though numerous estimations of the degrees of susceptibility are made and comparisons are drawn, nowhere, in either the report or appendix, is any attempt made to suggest an explanation of these differences. Nevertheless, when all the results scattered through the volumes are collected and compared, an explanation suggests itself which seems to accord fully with all the facts observed.

Let us begin by describing briefly what the results obtained with different classes of animals were.

It will be recalled that two separate and distinct investigations were carried on: one into the effects produced upon animals by the bacillus of bovine tuberculosis, the other into the effects produced by the bacillus of human tuberculosis. In each series experiments were made both by injecting cultures and emulsions of tuberculous lesions, and by feeding the

animals upon infected material. Some of the animals so treated died of generalised tuberculosis. Those which survived were killed after the lapse of a certain interval of time, and their organs and tissues were examined for signs of the disease.

In the first series of experiments it was found that rats and mice display the highest powers of resistance. Their capacity for overcoming the disease is enormous. The injection of small or moderate doses of the virus produced no effect at all. Large doses were only sometimes fatal, and even in these cases tuberculous lesions were not found on post-mortem examination, death having resulted from physiological changes brought about by multiplication of the bacilli in all the organs and tissues. Feeding experiments equally demonstrated a high degree of immunity. Twenty-six rats were fed with large quantities of highly infected material for periods of from twelve to twenty-three days, so that they received an enormous dose. On examination, two displayed minute traces of the disease, the rest were unaffected.

Dogs come next in order of power of resistance. They are very little susceptible to the bacillus of bovine tuberculosis either by subcutaneous inoculation or by feeding. They are more susceptible to intraperitoneal inoculation.

In cats the results of feeding were very similar to those obtained with dogs. They displayed, however, a smaller power of resistance to the effects of inoculation. Kittens were more susceptible than cats.

Next comes a group consisting of goats, calves, and pigs, between which no very great differences in the degrees of susceptibility can be observed, all being readily infected by moderate doses of the virus either by inoculation or by feeding. Frequently, however, the disease was not fatal. Calves were slightly less susceptible than goats, and goats less susceptible than pigs.

At the end of the scale come rabbits, monkeys, anthropoid apes, and guinea-pigs. All these are extraordinarily sensitive to the action of the bacilli. In the case of rabbits an attempt to determine the minimum fatal dose of a culture showed that so small an amount as one ten-millionth of a milligramme produced generalised tuberculosis. A chimpanzee succumbed to .001 milligramme when injected subcutaneously, and two

others to .01 milligramme when given with food. The sensitiveness of guinea-pigs is so marked that inoculation of them by a suspected material may be used as a test for the presence of the bacilli when the latter are too few in number to be detected by microscopic examination.

In the second series of experiments—those made with tubercle bacilli of human origin—the viruses employed were divided into two groups, those of Group I. being much more virulent than those of Group II. The same species of animals were used, and the results obtained were very similar to those observed with bovine material. Rats and mice again displayed high powers of resistance. Infection did not cause visible tuberculous lesions; but after the introduction of large doses, multiplication of the bacilli took place, and death resulted from a condition which has been called “tuberculous septicæmia.”

Dogs showed only slight traces of the disease when inoculated or fed with the bacilli of Group II. The single experiment made with Group I. caused death from general tuberculosis.

The few observations made upon cats showed that the species was highly resistant to the bacilli of Group II., but was more susceptible to those of Group I.

The goat, pig, ox, and rabbit were highly susceptible to the viruses belonging to Group I., but in most cases only showed slight disease after infection by Group II. Rabbits, however, were less resistant than calves, and with some viruses of Group II. showed progressive generalised tuberculosis.

Chimpanzees, monkeys, and guinea-pigs were readily infected by the bacilli of either group, and this in very small doses.

Combining the results from these two sets of experiments, and sinking small differences, we find that the order into which the various classes of animals fall when arranged according to their degrees of susceptibility to tuberculosis is as follows :

Rats and Mice.

Dogs.

Cats.

Pigs, Goats, Bovines.

Rabbits.

Guinea-pigs, Monkeys, Apes.

What explanations can be suggested to account for the enormous differences between the first and last of this series? In the first place, it seems clear that the degree of resistance

cannot be a function of the natural order; for we find at one end of the scale animals so dissimilar as rats and dogs resembling each other in their high degree of immunity, and, at the other end, guinea-pigs and chimpanzees alike exhibiting marked susceptibility. On the other hand, animals belonging to the same natural order—rodentia—occupy positions at the extreme ends of the scale. The Commissioners themselves do not appear to have recognised fully the complete absence of relationship between natural order and degree of susceptibility, for they remark on p. 14 of the report "the fact that the bacillus of bovine tuberculosis can readily by feeding as well as by subcutaneous injection give rise to generalised tuberculosis in the anthropoid ape, so nearly related to man, . . . has an importance so obvious that it need not be dwelt upon." Yet the experiments clearly establish that natural order has no significance whatever in this connection.

More importance might be attached to the question of diet, for carnivorous animals appear to have distinctly greater powers of resistance than herbivorous.

Dr. Archdall Reid has, however, furnished us with an explanation of the different degrees of susceptibility to tuberculosis exhibited by different branches of the human family, which, when extended to animals, will be found to agree in a remarkable manner with the facts. He has investigated the prevalence of zymotic diseases among a large number of races both of the Old and New Worlds, and has shown conclusively that a community exhibits a resistance to a disease which is in strict proportion to its past experience of it. The European peoples have suffered from tuberculosis at least since the days of Hippocrates, and they have now, in spite of the high death-rate which they still show, acquired a degree of immunity which can only be measured by observing the effects of the disease upon virgin soil. The history of the peoples of the New World furnishes an opportunity. Tuberculosis was carried thither in the sixteenth century, and among an unprotected native populace its effects were tragic. Whole tribes disappeared before it. The Caribbean and Tasmanian are gone. The Red Man only lingers on in spots artificially isolated from the contact of civilisation.

Such a relationship between experience and disease is fully in accord with the theory of evolution by natural selection.

A zymotic disease which persists from generation to generation and is not merely of occasional occurrence, eliminates in each generation those individuals who are most susceptible to it, or who are unable to develop adequate powers of resistance when attacked. Hence a progressively increasing degree of immunity and a continuously falling death-rate. Many other instances of this phenomenon have been investigated by Dr. Archdall Reid. Malaria is deadly to Europeans, but comparatively innocuous to West African natives who have undergone evolution against it; measles causes a high mortality among the Polynesians; Esquimaux die from vaccination, while Europeans usually recover from small-pox. Alcoholism, which he regards as governed by the same influences, is least among the ancient peoples round the Mediterranean who have had alcohol longest; greater among northern Europeans; and rampant among the primitive peoples of Australia and America to whom the poison is new.

If, with these facts in mind, we now look again at the list of animals arrived at above, we see at once that it corresponds exactly with that of the degree of closeness of their association with man, *i.e.* with the opportunities they have had in the past of acquiring tuberculosis from him. Rats and mice, which come first, though not ordinarily regarded as domesticated, are nevertheless very highly so from the scientific point of view. They are constantly and intimately associated with man. They inhabit his cellars and his sewers; they feed upon his refuse and the infected sweepings of his streets and pavements, which he persists in regarding as the appropriate place for expectoration. In such a habitat they must have undergone stringent natural selection against tuberculosis, the effects of which are seen in the extraordinary degree of immunity they now possess.

Dogs and cats come next. These animals have for long been closely associated with man, but not under such adverse conditions as rats. They inhabit the better ventilated parts of his dwellings, and are cleaner feeders. They are following the same path as the rats, but have not yet developed the high powers of resistance possessed by the latter. The greater susceptibility shown by the cat as compared with the dog is probably to be explained by their past histories. It is not possible to say positively which has been domesticated

the longer, for though the bones of both have been found associated with the remains of prehistoric man, this may merely indicate that they were used for food, and representations of both animals are found upon the ancient Egyptian monuments. Yet there can be little doubt that the cat is nearer the original wild form than the dog. This is shown by its instincts and habits. The cat, for instance, never exhibits the same devotion to men that the dog does; and, as Darwin points out, cats are always eager to attack poultry, while dogs, even when quite young, rarely show such a tendency. Further, the nocturnal habits of the domesticated cat, and the survival of the wild cat, enabled frequent crossings to occur long after such a process became rare or impossible for dogs.

Next comes a group comprising some of our familiar domesticated cattle, viz. goats, pigs, cows, and calves. These animals are only at times under adverse conditions. Whilst in the fields they enjoy their natural environment. But when they are shut up in stalls and byres they breathe a confined and tainted atmosphere, they are brought in contact with man, they risk catching his diseases. Under such circumstances the struggle has been only moderately severe, and the susceptibility of these animals to tuberculosis accordingly remains considerable.

At the end of the list are rabbits, monkeys, apes, and guinea-pigs, all wild forms, for the last, though now bred in captivity, was not introduced into Europe from South America until the sixteenth century. In their native forests and prairies these animals have never felt the touch of civilisation. They have never been subjected to any selection against tuberculosis, and, consequently, in their totally unprotected state, the disease attacks them with terrible virulence.

One point more may be mentioned. When both adult and young forms of the same species were examined, it was invariably found that the latter displayed greater susceptibility than the former. Thus calves were more susceptible than cows, kittens than cats, and puppies than dogs. This again agrees with what is found in the human species, for, with the exception of pulmonary tuberculosis, all forms of the disease are of much greater prevalence among children than among adults; and as regards such affections as measles and scarlet fever, their enormously greater prevalence among the young

has led to their being spoken of as "children's diseases." This lends further support to the theory of the gradual acquirement of immunity by natural selection, for, as Dr. Archdall Reid points out, "children represent a stage in the life history of the race when the general evolution against disease was not so advanced."

The gradual increase of immunity in the series of animals described above seems to afford a striking instance of progressive adaptation. It would be interesting, and would serve as some test of the theory, to know how far the relationship holds among animals other than those examined by the Committee. We should, for example, expect horses and sheep to resemble generally bovines and pigs; while deer, foxes, and squirrels should, like rabbits, be highly susceptible. Of particular interest would be some experiments on voles and other forms nearly allied to rats, but living in a different environment.

It is difficult to regard the variation upon which natural selection has acted in bringing about this evolution as other than "continuous." The observation, therefore, has additional value at a time when adherents of the advanced Mendelian school are suggesting that discontinuous variations have alone played a part in causing evolutionary changes. From the practical point of view the lesson taught is that while we may endeavour to maintain the health of our domesticated cattle by surrounding them with sanitary conditions, every effort should be made to discourage breeding from tainted individuals. It is only by assisting nature that we shall ever be able to call into existence naturally resistant stocks.

CAMPHOR, NATURAL AND SYNTHETIC

BY PERCY A. HOUSEMAN, Ph.D. (Wurz), A.I.C.

Chemist to the British Camphor Co., Ltd.

HISTORY AND OCCURRENCE

THE history of camphor reaches back at least fifteen hundred years. Two distinct varieties of the drug exist. The true—Japanese—camphor occurs in a tree (*Cinnamomum Camphora*) belonging to the laurel family indigenous to China and Japan, while Borneo camphor, which is known chemically as borneol, is found in *Dryobalanops aromatica*, one of the *Dipterocarpaceae*, which grows in Borneo and Sumatra. The earlier accounts of camphor refer to the latter variety. Many attempts have been made to introduce the camphor tree into other countries,¹ with a view to camphor production, but the experiments are very hazardous, for though the tree itself thrives well in almost any subtropical country, about fifty years must elapse before a good yield of camphor is produced. Moreover, the amount of camphor produced by each tree varies within very wide limits, the cause of this great variability not being understood. Since the Sino-Japanese War of 1894 the bad administration of the Chinese has been largely replaced by an efficient Japanese administration, whose monopoly of the supply of natural camphor is therefore scarcely likely to be disturbed for a long time. Their only competitor is the synthetic article, which has been produced on a commercial scale for about five years past. The following tables show the amount and value of the camphor exported from China and Japan since 1905 :²

¹ See e.g. *Board of Trade Journal*, July 18, 1907 (camphor trees in Federated Malay States); Colonial Office Report, 1907, Ann. No. 527 (camphor trees in Ceylon); Battandier, *J. pharm. chim.* 1907 (25), 182-3 (camphor trees in Algeria); *Chemist and Druggist*, 1907, 109 (camphor trees in the United States).

² These statistics have been compiled on the basis 1 kin = 1·32 lb. avoirdupois; 1 yen = 2s. 0½d.; 1 picul = 133·3 lb. avoirdupois; 1 Haikwan tael = 3s. 3½d. For the figures taken from the official Japanese and Chinese trade reports I am indebted to the Secretary of the London Chamber of Commerce.

Japan

	1907.		1906.		1905.	
	lb.	shillings.	lb	shillings.	lb.	shillings.
United States . .	1,756,000	3,446,740	1,491,800	2,484,050	1,252,400	2,010,390
Germany	977,000	2,655,150	529,400	1,039,400	117,500	234,630
British India . .	552,000	2,181,490	442,100	1,339,940	473,300	1,071,540
France	529,000	1,233,660	929,000	2,182,060	761,200	1,169,280
Great Britain . .	114,700	323,650	68,900	212,760	315,000	528,300
Australia	71,600	256,350	15,200	52,450	19,100	48,800
Hong Kong . . .	17,100	64,470	2,270	7,560	10,200	23,820
Straits Settlements	12,200	46,460	10,150	33,260	14,000	32,420
China	4,200	12,460	6,800	13,370	33,200	59,130
British America .	4,100	12,530	12,900	28,360	19,600	41,800
Other countries .	6,200	21,670	5,500	17,680	6,000	15,020
Total	4,044,100	10,254,630	3,514,020	7,410,890	3,021,500	5,235,130

China

	1906.		1905.	
	lb.	shillings.	lb.	shillings.
Hong Kong . . .	1,810,000	3,990,090	682,270	1,157,240
Great Britain . .	157,860	364,690	2,670	5,190
France	3,870	6,350		
United States . .	1,600	2,410	30,100	50,470
Germany	930	1,520		
Belgium	1,470	2,440		
Singapore; Straits	670	910		
Siam	530	770		
Dutch Indies . .	130	130		
Total ¹	1,977,060	4,369,310	715,040	1,212,900

Japan thus exported in 1907 1,805 tons of total value £512,730 (average value 2s. 6d. per lb.); in 1906 1,570 tons, of value £370,545 (average 2s. 1d. per lb.); in 1905 1,350 tons, of value £261,756 (average 1s. 9d. per lb.). China exported in 1906 882 tons, of average value 2s. 3d. per lb., and in 1905 only 320 tons, of average value 1s. 8d. per lb.

EXTRACTION AND REFINING

The extraction of the camphor from the tree is a very simple process. The wood, cut up into chips, is subjected to the action of steam, whereby camphor, mixed with various essential oils, is driven off and condensed in a cooled chamber. The crude Japanese camphor, a coarse greyish powder, is exported in two grades—Samuel A and Samuel B—and is then refined by means of steam distillation or by sublimation or crystallisation. Refined

¹ Including re-exports.

camphor comes into the market in three forms—as “flowers,” obtained by condensing the sublimed camphor in a chamber; as “bells,” which are hollow transparent domes of camphor, weighing usually from 2 to 6 lb., and made by subliming in glass retorts; and as “blocks,” obtained by subjecting the flowers to strong pressure. It is in this last form that one is accustomed to see camphor. A by-product in the extraction of camphor is the “camphor oil,” which contains many essential oils as well as camphor in solution.

PROPERTIES AND USES

Camphor is a colourless crystalline substance melting at 175°C. , but is volatile even at ordinary temperature. It has a characteristic odour which is said to be detectable at a dilution of '00005 gram of camphor in 1 litre of air. It is readily inflammable, burning with a smoky flame. In water camphor is almost insoluble, one part of camphor requiring about 1000 parts of cold water for solution (*aqua camphoræ*). In all organic solvents it dissolves with ease. Japanese camphor has a strongly dextro-rotatory action on the plane of polarised light— $\alpha_{(D)}$ in alcoholic solution $\pm 44.2^{\circ}$. The lævo-rotatory camphor occurs in nature in the oil of *Matricaria parthenum*, and is known as matricaria camphor. With the exception of its opposite rotatory power it is identical with ordinary Japan camphor.

Camphor finds application chiefly in three directions:

1. In pharmacy. Camphor is used in more than thirty different forms. It is antispasmodic, a stimulant, cardiac, and local anodyne, and is used in fevers, hysteria, dysmenorrhea, colic, etc. The familiar “camphorated oil” is a solution of one part of camphor in about four parts of olive oil.

2. In explosives. The use of camphor in explosives would appear to be somewhat limited. It exercises a preservative action on blasting gelatine.

3. In the manufacture of celluloid (xylonite). This industry consumes the greater part of the camphor produced. Celluloid was invented in 1869 by Daniell Spill in England and by Hyatt in America. The process of manufacture, expressed in a few words, consists of the incorporation of camphor with nitrocellulose (pyroxylin, collodion wool, guncotton) and a suitable solvent such as acetone. The enormous number of articles now made of celluloid indicates what a large quantity of camphor must be

consumed by this important industry. The presence of 0.1 per cent. chlorine remaining in synthetic camphor is said to render it unsuitable for the manufacture of celluloid, so that manufacturers of synthetic camphor are set a sufficiently high standard of purity.

THE CHEMISTRY AND CONSTITUTION OF CAMPHOR

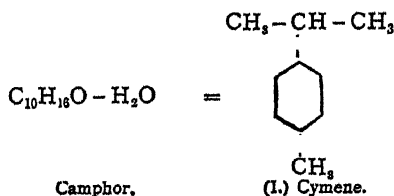
Camphor is a derivative of the important class of organic compounds called terpenes, the parent substances of which are unsaturated hydrocarbons possessing the empirical formula C_5H_8 . The terpenes are divided into four classes:

1. Hemiterpenes, C_5H_8 .
2. Terpenes, $C_{10}H_{16}$.
3. Sesquiterpenes, $C_{15}H_{24}$.
4. Polyterpenes, $(C_5H_8)_x$.

The second class is the most important, and it is to this class that camphor belongs. In considering the constitution of camphor—a question which has occupied the attention of chemists for many years—we must briefly review its most important reactions and decompositions, and then pass on to its synthesis from pinene.

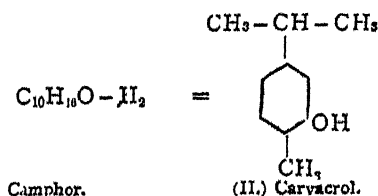
Camphor is a saturated compound having the formula $C_{10}H_{16}O$.

It is a ketone; *i.e.* contains the group >C-CO-C< . The evidence for this consists in its ability to react with hydroxylamine forming an oxime (camphoroxime $C_{10}H_{16} = NOH$). Further it is reduced (by sodium and alcohol, or electrolytically) to borneol, which possesses the character of a secondary alcohol, $C_9H_{16} = CHOH$. When distilled with phosphorus pentoxide, camphor yields the hydrocarbon cymene $C_{10}H_{14}$ (I.), whose constitution is known to be para-methyl-isopropyl-benzene.



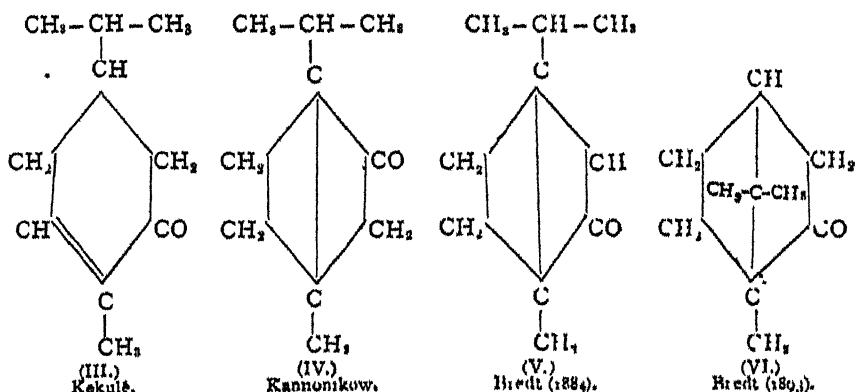
This same cymene complex (a benzene nucleus with methyl and isopropyl in para position to one another) is therefore assumed to be present also in camphor. On boiling with iodine,

camphor is converted to carvacrol (II.), which is also of known structure—



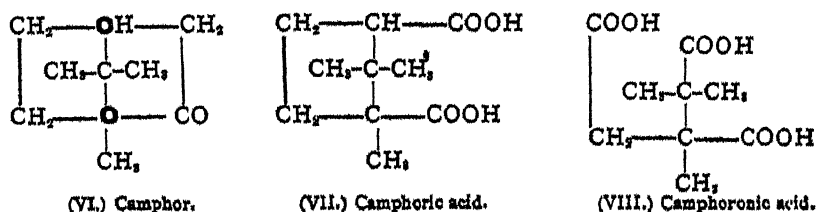
All the above reactions are satisfied by the camphor formula (III.) proposed by Kekulé in 1873. It does not, however, account for the saturated character (lack of additive power) in camphor, nor for its oxidation, which is of great importance for the determination of its constitutional formula.

Formula IV. was put forward by Kannonikow in 1883, and V. by Bredt in 1884.

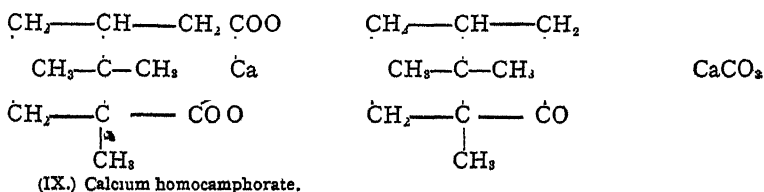


That Bredt was correct and Kannonikow incorrect in the position of the carbonyl (CO) group is shown by the conversion of camphor to carvacrol considered above. Finally, Bredt modified his formula in 1893 to the form shown in VI.

The oxidation of camphor to camphoric acid (VII.) and camphoronic acid (VIII.) is then formulated as follows :



Camphoric acid is of known structure. It is α - α - β -trimethyl-tricarballic acid, for on dry distillation it is broken down to trimethyl-succinic acid, isobutyric acid, and carbon dioxide. It has, moreover, been prepared synthetically with the aid of acetoacetic-ether. All these reactions find a satisfactory explanation in Bredt's formula for camphor, which is therefore now generally accepted. Lastly, two other points may be mentioned in connection with this difficult question of the structure of the camphor molecule. The first is the partial synthesis of camphor by the distillation of the calcium salt of homocamphoric acid (IX.),¹ which is shown thus—



The second is the support derived from stereo-chemical evidence. It will be seen by reference to formula VI. that camphor contains two asymmetric carbon atoms (printed in heavy type)—*i.e.* two carbon atoms, each of which is combined to four different groups. To these asymmetric carbon atoms camphor owes its property of optical activity. If now the carbonyl (CO) group be reduced to CH_2 , the asymmetry of both carbon atoms is destroyed. If therefore the structure for camphor assumed by Bredt be the correct one, the optically active camphor must become inactive upon substituting CH_2 for CO. This is entirely supported by the facts.

SYNTHETIC CAMPHOR

Synthetic camphor is identical with the naturally occurring product in all its properties, chemical and physical, with the exception that it has no rotatory action on the plane of polarised light. The "inactive" camphor which is obtained synthetically is a mixture of the dextro- and lævo-forms, and has not yet been resolved into its optically active components, although Pickard and Littlebury² have recently accomplished the resolution of inactive borneol into its dextro- and lævo-modifications

¹ Bredt and Rosenberg, *Annalen der Chemie*, **289**, 1.

² *Trans. Chem. Soc.* **91** (1907), 1973.

by combining acid bornyl phthalate with an alkaloid, separating the active salts by fractional crystallisation, and then saponifying each, whereby the optically active modifications of borneol are obtained.

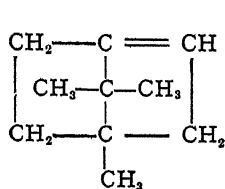
It is necessary to keep the true synthetic camphor quite distinct from the so-called "artificial" camphor, which indeed is not camphor at all, but simply the hydrochloride of the terpene pinene, $C_{10}H_{16} \cdot HCl$. Its odour slightly resembles that of camphor, but it is of course a totally different substance. The synthetic production of camphor starts out from turpentine, the resinous liquid which exudes from various *Coniferae*.¹ When turpentine is distilled, oil of turpentine passes over and the resin colophony remains behind. The oil of turpentine is then carefully fractionally distilled. The portion boiling at $155-160^{\circ} C.$ (90 per cent. of a good sample) consists chiefly of the hydrocarbon pinene, $C_{10}H_{16}$ (formula X.). Well cooled pinene, when saturated with dry hydrochloric acid gas, is converted into pinene hydrochloride (XI.) (the "artificial camphor" above noted), which is thrown out as a white crystalline mass. The next step in the process consists in splitting off hydrochloric acid from the pinene hydrochloride. A hydrocarbon $C_{10}H_{16}$ is obtained, not however the pinene from which we started, but the isomeric camphene (formula XII.). It will be observed that the double bond has moved from the position 1:2 to the position 2:3; *i.e.* the hydrochloric acid has been split off from a different part of the molecule from that which it entered. It is worthy of note that camphene is the only solid terpene hydrocarbon. It is a white solid, melting at $50^{\circ} C.$ This elimination of hydrochloric acid and consequent conversion of pinene hydrochloride to camphene may be accomplished in a great variety of ways. A few may be mentioned:

1. Heating with ammonia or with organic bases such as methylamine, pyridine, quinoline, piperidine, piperazine.
2. Heating with caustic soda and a salt of a higher fatty acid (soap), such as stearates of sodium, lead, copper, etc.
3. Heating with phenolates or naphtholates of sodium, potassium, calcium, magnesium, etc.

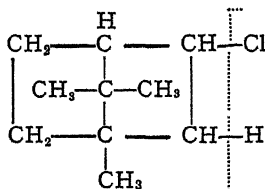
¹ The pine species which provide most of the turpentine are—in America *Pinus palustris*, in France *Pinus maritima*, and in Russia *Pinus sylvestris*. Russian turpentine contains much less pinene than French or American, and is not suitable for making pinene hydrochloride.

4. Heating with anhydrous acetates of lead, zinc, or alkaline earth metals in glacial acetic acid solution.

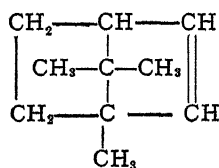
These methods, and many others, yield camphene of varying degrees of purity; method 3 gives a very pure camphene, while the use of metallic acetates yields a mixture of camphene with bornyl acetate and other bodies.



(X.) Pinene.



(XI.) Pinene hydrochloride.

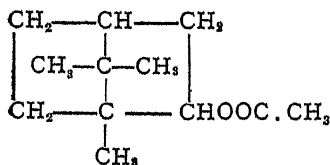


(XII.) Camphene.

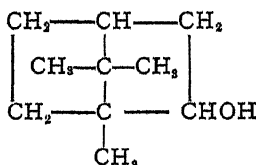
The next stage in the process is the so-called "esterification"—the conversion of camphene into bornyl acetate (XIII.). Bertram and Walbaum¹ found that when camphene is dissolved in about its own volume of glacial acetic acid and allowed to stand at slightly elevated temperature with about 5 per cent. of sulphuric acid, it adds on the elements of acetic acid and is converted into bornyl acetate.² Bornyl acetate is an oil boiling with some decomposition at about 225° C.; it distils at 107° C. at a pressure of 13 mm.; it is readily volatile in steam.

The bornyl acetate is now saponified, whereby borneol (XIV.) is obtained. This is accomplished by treatment with alcoholic caustic soda, sodium acetate resulting as a by-product. The resultant borneol is, with the exception of its optical inactivity, identical with the naturally occurring Borneo camphor.

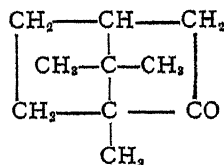
The final operation in the synthesis consists in the oxidation of the borneol to camphor.



(XIII.) Bornyl acetate.



(XIV.) Borneol.



(XV.) Camphor.

¹ Jour. f. prak. Chem., N.F. 49, 1.

² In the synthesis of camphor it is really *iso*-bornyl acetate and *iso*-borneol that are obtained. The differences between the ordinary and *iso*-compounds are slight. The nature of the isomerism is not clear. It is not one of position, for both borneol and isoborneol result from the reduction of camphor. Moreover, isoborneol yields ordinary camphor on oxidation. For simplicity, therefore, the terms "bornyl acetate" and "borneol" are employed throughout this article.

Here again a variety of substances are employed—air, ozone, chlorine, nitric acid, potassium dichromate and sulphuric acid, potassium permanganate, and many others. Lastly, the crude camphor must be refined by one of the methods already described for natural camphor; *i.e.* by steam distillation, sublimation, or crystallisation.

The patents in connection with the manufacture of synthetic camphor, of which a considerable number are being taken out at the present time, consist chiefly of—

- (1) Variations in the manner of splitting off hydrochloric acid from pinene hydrochloride.
- (2) The use of other esters than the acetate of borneol, *e.g.* formate, oxalate.
- (3) Different oxidising agents for the conversion of borneol to camphor.

A few patents seek, by heating with organic acids, to pass direct from pinene to borneol without passing through the pinene hydrochloride stage, but those processes which adopt the course detailed above seem to be the best.

The synthetic production of camphor thus involves a series of six distinct operations before the final article is obtained—turpentine → pinene → pinene hydrochloride → camphene → bornylacetate → borneol → camphor.

Synthetic camphor is now being manufactured by a number of firms. It represents one of the best and most recent examples of the growing tendency (due to scarcity of raw materials) to replace natural products by artificially prepared substances, which are either identical with, or can be used as substitutes for, the naturally occurring body. Indiarubber presents a similar problem not yet successfully solved.

Assuming a good supply of turpentine at moderate price, there is little doubt that synthetic camphor which is quite equal in all respects to its Oriental rival has a good future before it, and will be able to compete successfully with Japanese camphor.

THE TRANSFORMATION OF ELEMENTS

PART II. RADIUM AND ITS EMANATION

By A. T. CAMERON, M.A., B.Sc.
University College, London

THE first part of this paper contained a brief account of the methods by which the radioactive elements had been discovered, and of their known properties. The fact that radium compounds have been isolated in a state of comparative purity, and in amounts much larger than those of the other active elements, has led to a more complete study of this element. The data have accumulated to such an extent as to demand separate treatment.

The amount of radium salts actually prepared in any state approaching purity is probably about five grams. It is in existence chiefly as bromide and chloride. Twenty or thirty tons of pitchblende—from the mines at Joachimsthal in Bohemia—were worked up in order to obtain this extremely small quantity. Only three or four investigators have quantities of half a gram or more; most of the radioactive experiments have been carried out with quantities of from two or three to fifty or sixty milligrams. It is impossible to obtain any appreciable quantity, not only on account of its price—at present about £15 a milligram—but especially because of the limited amount which has as yet been prepared. The output of the Joachimsthal mine is controlled by the Vienna Academy of Science, to whom much credit is due for its loans of large quantities of radium preparations to several of the most distinguished workers in this line of research, whereby much important work has been accomplished.

The chloride, bromide, sulphate, and carbonate are known. The metal has not yet been isolated; this is due both to the necessary minuteness of the operations and the great risk of loss. The salts strongly resemble those of the alkaline earths, especially barium, and are given corresponding formulæ. The sulphate, RaSO_4 , is even more insoluble than barium sulphate;

the crystallised bromide, assumed to be $\text{RaBr}_2 \cdot 2\text{H}_2\text{O}$, is somewhat less soluble than $\text{BaBr}_2 \cdot 2\text{H}_2\text{O}$. The salts colour a flame crimson, as do strontium salts. The spectrum was first examined by Demarçay, and has more recently been studied by a number of other physicists. In its general aspect it resembles the spectra of the other alkaline earth metals.

The salts are quite white when freshly prepared; they closely resemble barium and calcium salts. On long standing they turn first yellow, and finally brown. It is found that the colour is much deeper with impure preparations; pure salts are scarcely coloured after a considerable period. This can be used as a test of the degree of purity.

All the radium salts are radioactive; it is practically certain that the metal would exhibit the same radioactive properties and disintegrate in the same way. There are several grounds for this statement. Uranium has the same radioactive properties, whether free in the state of metal or combined in its salts. When a solution of radium bromide is electrolysed, the mercury becomes permanently active, showing the formation of radium amalgam. This shows the same radioactive properties of its salts. Several radioactive elements have been deposited on metals, that is, they have been deposited in metallic form themselves. One example is polonium or radium F, separated by placing a rod of bismuth in a solution of its salt; it possesses the same activity as metal as it does in solution.

Direct determinations of the atomic weight of radium have been made by Madame Curie. In her initial experiments, by successive crystallisations of barium chloride she obtained fractions increasing in atomic weight from 137.5 (barium) to 225. The final fraction weighed only 0.1 gram. Much later, in 1907, she worked up 0.4 gram of chloride of such purity that its spark-spectrum showed that only minute traces of barium were present. By precipitating the chloride as silver chloride with warm silver nitrate solution the ratio $\text{Ra} : \text{Ag}$ was obtained. Three successive determinations of the atomic weight with the same fraction gave as mean value 226.18. Madame Curie concluded that the atomic weight is 226.2 ± 0.5 .

Quite recently Thorpe has published the results of his determinations, using Madame Curie's methods. The spectrum of his final preparation of radium chloride scarcely showed the presence of the strongest barium lines. Three successive

determinations of the chlorine as silver chloride made the atomic weight of radium respectively 226·8, 225·7, and 227·7 (mean 226·7). Until further work has been completed the figure 226·5 may be accepted as approximately correct.

From certain series of lines in the spectrum, analogous with those of calcium, strontium, and barium, indirect determinations of the atomic weight have been made by Runge and Precht, and by Marshall Watts; the former deduce an atomic weight of 258, the latter a weight agreeing with Madame Curie's figure.

Although the time of decay of radium is extremely slow, there is some doubt as to its actual duration; the matter will be discussed later. The product of the decay is that gas or emanation discovered by Dorn in 1900. It can be pumped off from radium preparations or carried away by a current of air; obtained in this way its rate of decay has been examined by a number of investigators. Rutherford and Soddy's first determination was 3·71 days (half life); the Curies obtained the figure 3·99 days, Sackur 3·86. Recently (in 1907) Rümelin, working under Rutherford, obtained the value 3·75 days (the mean of eight experiments; the limits were 3·70–3·80).

On account of the ease with which small quantities of gas can be separated, tested, and purified in comparison with small quantities of a solid, and of the fact that this is the only emanation which has an appreciable life (the half-life period of thorium emanation is 54 seconds, of actinium emanation 4 seconds), it has been examined more completely than any other radioactive substance.

Rutherford and Soddy, and later, Ramsay and Soddy, demonstrated beyond all doubt its peculiar inertness by subjecting it to a number of extremely violent chemical tests, measuring its radioactivity qualitatively and quantitatively before and after experiment, to determine whether any change had been effected. It is not altered by passage, mixed with oxygen, over platinum black at the highest attainable temperature, nor by passage over red-hot lead chromate, nor by red-hot magnesium powder, magnesium lime, or zinc dust. Sparking with oxygen over alkali produces no change; neither does ignition of phosphorus in that oxygen. It will combine with no other element under any conditions hitherto employed. The only other gases known which behave in a similar manner

are those of the argon series and the thorium emanation. There exists strong evidence, therefore, for the belief that these two emanations are higher members of the argon series.

Dry radium preparations evolve only a small amount of their emanation. The rest is occluded, and decays in that condition; the subsequent products of decay remain mixed with the radium salt. The whole of the emanation can be driven off by dissolving the radium salt in water, and creating a vacuum above the water surface, or a great part by heating the dry salt. Kolowrat has studied the latter subject in detail, using barium chloride containing radium. He finds that from ordinary temperatures up to 350° the amount of emanation given off is constant and only 1 per cent. of the whole; above that temperature it increases rapidly, 90 per cent. being evolved at 830° . The amount then falls to 60 per cent. at 920° , and rises finally to 100 per cent. at the melting point of the salt, about 945° . Similar results were obtained with the fluoride. He concludes that it is impossible to estimate the radium present in a solid substance by evolution of the emanation unless the substance is fused.

Henriot has recently published (1908) the results of a research on the analogous problem of the absorption of emanation by charcoal. At ordinary temperatures it is completely absorbed; at higher temperatures the amount gradually decreases, and by heating the charcoal to dull redness the last traces of emanation are expelled.

Kofler has determined the solubility of the emanation in water and various solutions. The coefficient of absorption—determined by radioactive measurements—in water at 18° is 0.270. The presence of dissolved salts lessens the solubility. Hofman, previously, using similar methods, determined the solubility at the temperatures 0° , 20° , 40° , and 60° . The coefficient of absorption was in the respective cases 0.52, 0.28, 0.16, 0.12. It is much larger in such solvents as petroleum and toluol.

Attempts have been made to estimate its atomic weight by an indirect method, the rate of diffusion. Rutherford and Miss Brooks, and later, Makower obtained results leading to a molecular weight of about 100; Bumstead and Wheeler, by comparing its rate with that of carbon dioxide through a porous plate, obtained the figure 180; the amount of diffusion of ema-

nation in any time was estimated by the increase of activity of the gas into which it diffused. Since in the argon series the molecular and atomic weights appear to be identical, their evidence goes to show that the atomic weight of the emanation is of the order 200.

On the assumption that the emanation occupies a space in the periodic series twice as much above xenon as xenon is above krypton, the intermediate space corresponding to the figure 173 being unfilled, Ramsay has calculated that its atomic weight should be 216.5. If an α particle consists of a helium atom, and if the atomic weight of radium is 226.5, then, since radium and its emanation appear to be consecutive products, the atomic weight of the latter should be 222.5. Rutherford and Soddy found that the emanation condenses to liquid or solid form at about -155° C. Henriot states that the solidified emanation commences to volatilise between -150° and -160° , at a few millimetres pressure only. It should be pointed out that this figure by no means agrees with the supposition that it is a gas of the argon series of atomic weight about 220. Xenon (atomic weight 128) melts at -140° , and by extrapolation the value for an element of the series of atomic weight 220 should be in the neighbourhood of -60° .

Radium emanation can be readily condensed by the use of liquid air. In order to show how it is isolated, and its properties examined, a somewhat detailed account will now be given of the methods employed by Ramsay, in his researches on this gas with Collie, and with Soddy and Cameron.

The radium salt is dissolved in water in a small bulb A (fig. 1), connected through the trap B with the intake tube of a mercury Töpler pump. The tap C prevents the mercury flowing back into the bulb, the trap B catching any that is accidentally allowed to pass the tap. The bulb and tubes are initially pumped empty of air; the mercury is forced past the valve D (by raising the reservoir E sufficiently and closing the outlet tube F) and a little way past C, which is then closed. The mercury seal entirely prevents leakage through C. The emanation is allowed to collect during three or four days, and is then pumped off by repeatedly lowering the reservoir E, opening C for a few seconds, and forcing the gas which passes C through the outlet tube F into the test-tube G, previously filled with, and inverted over, mercury. The operation is continued

until a vacuum is produced. The emanation is not present in sufficient quantity to admit of its being pumped away by itself. But both radium salts and emanation decompose water, the property being probably due to the action of α rays. Hydrogen and oxygen are produced in comparatively large quantity. Sir William Ramsay found that as a mean of eight experiments varying from 48 to 336 hours, 32 cubic centimetres of the mixed gases are produced by 1 gram of radium bromide in 100 hours.

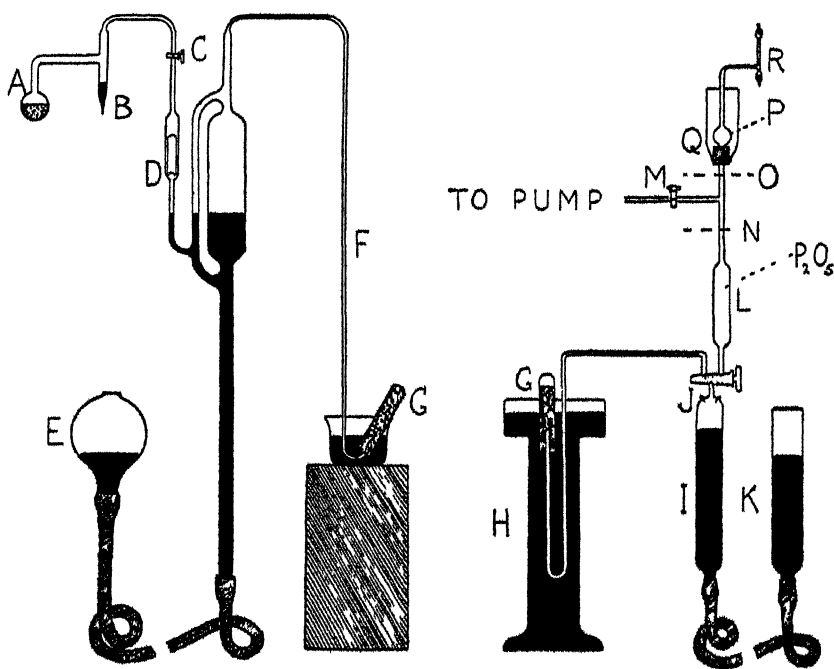


FIG. 1.

The quantity from 50 milligrams, 1.6 c.c., can quite easily be passed through the pump without loss. This is the gas which is actually collected in the test-tube; the emanation is carried along with it. There may also be present a very small trace of carbon dioxide.

There is one remarkable feature about the proportions of hydrogen and oxygen in the mixed gases. They never exactly correspond to those required for the formation of water; there is always some 3 to 10 per cent. of excess hydrogen. This excess has not yet been explained completely. Part, perhaps the whole, may be due to the formation of hydrogen peroxide in

the radium solution; this is known to take place to a slight extent. The excess of hydrogen, however, is extremely useful in enabling the emanation to be separated from admixed gases.

The test-tube *g*, containing these gases, is transferred to the mercury trough *h*, and forced through the inverted syphon, made of capillary tubing, and through the three-way tap into the burette *i*. At the top of the burette a little moist potassium hydroxide has been previously melted to the glass; this absorbs any trace of carbon dioxide. A spark from a coil is passed through the gas by means of the platinum wires at *j*. Immediate recombination takes place, and only the excess of hydrogen and the emanation remain, but in a moist condition owing to water formed by the explosion. The apparatus above the three-way tap is completely exhausted of air by a second pump, connected through the tap *m*. This tap is then closed, and the three-way tap opened. The reservoir *k* is raised and the gas forced through the three-way tap and the small bulb *l* containing phosphoric anhydride, in order to dry it completely. The mercury is then raised to *n*, and the three-way tap closed. Liquid air is poured into the cup *q*; the bulb *p* cools quickly to -185° , the emanation condensing within, while hydrogen remains gaseous at this temperature. After about five minutes the tap *m* is opened, and the hydrogen is completely pumped away. It has been found repeatedly that even at -185° the emanation has a slight vapour pressure; pumping must not be carried to excess or else emanation is lost. If the work is carried on in the dark, the pumping is stopped when the bubbles passing down the fall-tube of the pump commence to phosphoresce a little. The tap *m* is then closed and the mercury level raised to *o* to prevent leakage through *m*. During the whole of this time the cup *q* is kept filled with liquid air. After *m* is closed, the liquid air is allowed to evaporate, whereupon the emanation regains its gaseous condition and can be examined.

Using a somewhat similar method, Ramsay and Soddy succeeded in freeing the emanation from most of its impurities and forcing it into a minute spectrum tube such as *r* in fig. 1, which was then sealed off and examined. They were unsuccessful in obtaining the spectrum of the emanation; a trace of carbon dioxide masked it. On the other hand, they obtained the spectrum of helium, which rapidly grew brighter. By comparing its

intensity with that of other spectrum tubes containing known quantities of helium, they actually succeeded in obtaining an approximate measurement of the amount formed.

Ramsay and Collie, introducing the means already described to free the gas of last traces of carbon dioxide, succeeded in enclosing it in a similar spectrum tube and mapping its spectrum. This is distinct, establishing the emanation as a true element, its general features resemble those of the other gases in the argon series.

Replacing the spectrum tube by a capillary tube of very fine bore, whose volume measurements were determined subsequently, Ramsay and Soddy made two measurements of the actual amount of emanation produced. They used different capillary tubes and obtained different results in the two cases. In the first experiment there was a sudden large drop during the first day, the contraction thereafter being roughly proportional to the decay of activity. The gas completely disappeared by the end of a month. On heating the tube helium was given off from the walls and detected by its spectrum. In the second experiment the initial volume was much smaller. There was no contraction, but the gas expanded during a considerable time.

An attempt to explain these differences was made some years later by Ramsay and Cameron. On account of the *sticktion* of the mercury in its passage through the phosphoric anhydride tube they found it convenient to use a somewhat modified apparatus, a sketch of which is shown in fig. 2 (reproduced from the *J. Chem. Soc.* 1907, 91, 1907, by kind permission of the Secretaries). The whole apparatus was arranged by the side of a scale graduated in millimetres. The general arrangement was the same as that shown in fig. 1. A side tube connected with a second reservoir *m* was introduced; the emanation, having been isolated, was allowed to enter through the tap *k*, which was closed after forcing mercury through it so as completely to clear the tube of gas. The gas was forced up into the capillary tube and its volume read at different pressures (by raising and lowering the reservoir *m*, and noting the difference in the mercury levels). This was repeated daily for some time. Two glass capillary tubes were used, two sets of measurements being taken with each. Their diameters were respectively 0.448 and 0.453 millimetres. For the two final measurements a silica tube of 0.208 millimetres diameter was taken. Silica and glass cannot be joined

directly ; a ground-in joint was made (at o, fig. 2). Silica tubing possesses several advantages over glass for such measurements. Although the silica tube had less than half the diameter of the glass tubes, yet the *sticktion* of the mercury was negligible, while in the glass tubes it was so great that accurate readings could be obtained only with difficulty. Again, silica is less readily

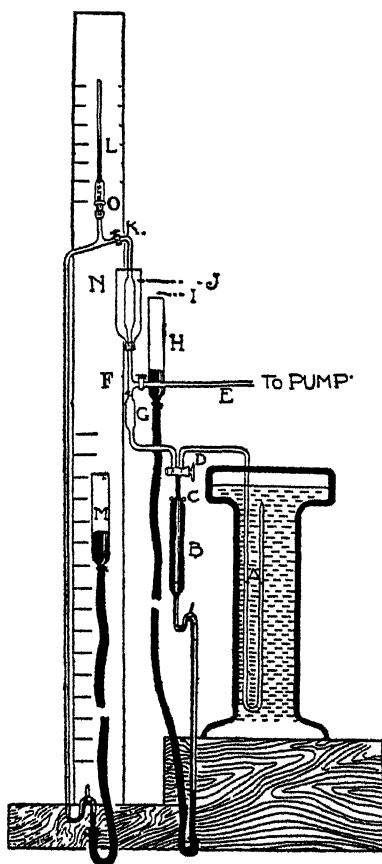


FIG. 2.

coloured than ordinary glass, pure silicon dioxide being unaffected. The first two capillary tubes were of potash glass, which is so much affected by the β rays that after one or two days the level of the mercury inside can no longer be observed. The effect was partly overcome by confining the emanation to the top of the tubes, except when actual readings were being taken.

The results obtained are here briefly summarised. The observation by Ramsay and Soddy, that the emanation obeyed Boyle's Law, was confirmed by numerous sets of measurements. The divergence of the results in different experiments is due to the nature of the glass capillary employed. The emanation forces a large part of the helium it produces into the walls of the tube. After some time, the disintegration products—radium D especially—appear to act as a shield. The helium is no longer forced into the walls, and the total volume increases. If the glass is unusually impermeable this expansion takes place from the beginning of the experiment, thus showing that the emanation produces more than its own volume of helium. Ramsay and Soddy found from their spectrum measurements that the ratio was about 3:1; the later measurements of the actual volumes confirmed this figure. This agrees with the fact that three α particles are produced by the time that radium D is formed.

The life of radium D is so long that at the end of a month, by which time less than 1 per cent. of the emanation remains, scarcely any of the after-products have been formed; while the lives of A, B, and C are of such short duration that they disappear completely within a few hours of their formation. After heating the measuring tubes to remove the colour, an extremely small amount of a brown deposit, possessing a submetallic lustre, has been observed; in all probability this is radium D.

On account of the slight volatility of the emanation at -185° (referred to previously) the experiments showed no very close agreement. The mean of the three which were considered most accurate gave the result that 87.7 milligrams of metallic radium (in solution as bromide) gave rise in 3.86 days (the half-life period of the emanation according to Sackur) to 0.310 cubic millimetres of emanation. This is the volume occupied by a small pin-head. Ramsay and Soddy's figure for the same quantity of radium was 0.222 cubic millimetres.

In addition Ramsay and Cameron observed a new change, quite different from ordinary disintegrations; it appeared to explain the large decrease in volume observed by Ramsay and Soddy in their first experiment. In the last four experiments, in which the actual initial volumes varied from 0.058 to 0.397 cubic millimetres, the volume contracted in about one and a

half hours to 50 per cent. of the original, within the limit of experimental error. In two experiments readings were taken every few minutes; the change in volume plotted against time gave an exponential curve, in that respect resembling ordinary radioactive changes. The volume change could be reversed by heating, for at 80° the gas expanded to within 10 per cent. of its original volume. After the rapid contraction was completed, a slow contraction began; this was proportional to the decay of radioactivity.

The authors attempted to explain this phenomenon by the supposition that the emanation at first was a monatomic gas, as are the other gases which it resembles; after the initial change was completed the gas had become diatomic ($2 \text{ Em}_1 = \text{Em}_2$) with consequent halving of volume. In this respect comparison may be made with nitrogen peroxide; at low temperatures its molecule is represented by the formula N_2O_4 , while heat converts it into NO_2 molecules. It is not yet known if this change occupies a measurable time.

It is necessary to refer in some detail to the heating effect of radioactive substances, radium and its emanation especially. P. Curie and Laborde first noted that a radium salt kept itself at a temperature several degrees higher than the surrounding air. They measured the amount of heat in a calorimeter, and calculated that one gram of pure radium chloride emits 100 gram-calories of heat per hour. Precht in 1906, using an ice calorimeter, calculated that one gram of anhydrous radium bromide emits nearly 80 gram-calories per hour. Rutherford and Barnes have shown that 75 per cent. of the heating effect is due to the emanation and its products; the total heat evolved during the life of a cubic centimetre of the gas is about seven million gram-calories. When the same volume of electrolytic gas (hydrogen and oxygen in the proportions to form water) is exploded, it evolves 3 calories; hence during its disintegration the emanation emits over two million times the amount of heat evolved by the explosion of an equal amount of electrolytic gas; the latter reaction gives rise to more heat than any other known chemical change.

Ramsay has shown qualitatively in a very interesting manner the production of heat from emanation. Two thermometers were taken, one an ordinary instrument reading to tenths of a degree; the bulb of the other was hollow; it was

standardised against the first. A mixture of hydrogen and emanation was then introduced into the hollow space in the bulb. The two thermometers were wrapped in cotton-wool and placed in silvered vacuum vessels; daily readings were taken for three weeks. The difference was 0.52° on the first day, rose to 0.73° on the second, then slowly grew less; the thermometers registered the same temperature at the end of a fortnight.

Pegram has shown that thorium also evolves a very small quantity of heat.

It has been shown by various experimenters that radium is scattered throughout the earth in minute quantity, though sufficiently large to account for the natural ionisation of the air. Some interesting speculations have been put forward by G. H. Darwin, Rutherford, Strutt, and others, connecting this universality of radioactive matter, each minute particle of which evolves a definite amount of heat, with the total heat of the earth. It has been shown that if radioactive matter exists in the same proportion throughout the earth as it does at the surface, then the heat it evolves is far more than is required to maintain the earth's present temperature. Consequently, various hypotheses have been suggested to show that radioactive matter may be non-existent, or unchanging, in the earth's interior. It also follows that the age of the earth may be much greater than 100,000,000 years, the figure calculated by Lord Kelvin from the observed temperature gradient at the earth's surface. Provided similar radioactive changes are taking place in the sun, it will emit heat at its present rate for a period many times greater than that obtained by calculations based on dynamical data.

The duration of the life of radium has been estimated in two ways. Rutherford has calculated it from an estimation of the number of atoms of radium disintegrating in one second. He has shown experimentally that 6.2×10^{10} α particles are expelled per second. From considerations of the heat effect he concluded that each atom in disintegrating emits one α particle. Hence 6.2×10^{10} atoms of radium break up per second. It has also been shown that one cubic centimetre of hydrogen at 0° C. and 760 mm. pressure contains 3.6×10^{19} molecules. Hence the number of atoms of radium considered as a monatomic gas under the same conditions can be calculated; if its atomic weight is

226.5, this number is 3.6×10^{21} . In one second therefore the fraction 1.95×10^{-11} of the total number of radium atoms disintegrates; in a year, of a gram of radium about half a milligram has disintegrated. The average life of radium is hence about 1,800 years; its half-life period 1,300 years.

Rutherford has more recently modified these figures, his later conclusions pointing to a half-life period of more than 2,000 years.

Ramsay and Soddy used somewhat similar reasoning; they assumed that one atom of radium produces one atom of emanation, and that both gases are monatomic. Applying this reasoning to the figures for the volume of emanation obtained by them, Ramsay and Cameron calculated that 1 gram of radium would produce in one day 1.162 cubic millimetres of emanation. Since 1 gram of radium, if monatomic, would occupy $(2 \times 11.2) \div 226.5 = 0.1$ litre = 10^5 cubic millimetres, the proportion of radium changing per day is 1.162×10^{-5} . The average life will be the reciprocal of this number, $\frac{10^5}{1.162}$ days or 236 years. No explanation of these different results has yet been put forward.

A fairly complete resumé of the chemical and physical properties of radium and radium emanation has now been given. Their action as chemical agents is in all probability entirely due to the radiations they emit. A brief mention of these effects has been given in Part I. (vol. ii. pp. 537-8). A more detailed statement follows.

Giesel was the first to observe that radium salts decompose the water in which they are dissolved. Davis and Edwards have noted a rapid combination of hydrogen and oxygen, when solid radium salt is actually placed in the mixed gases. Ramsay obtained a similar effect by adding emanation to electrolytic gas. When the radium salt is placed in a closed glass bulb, and this immersed in the liquid or gas under observation, any action which takes place must be due to β and γ rays only—derived from radium B and C—for all the α rays are absorbed by the glass envelope. Under such conditions Ramsay has shown that water is not decomposed; Jorissen and Ringer have observed no combination between hydrogen and oxygen. It therefore appears that β rays do not produce these changes; they are probably due to α rays.

The observed effects of β rays, as shown by the action of

radium salts enclosed in glass, may be summarised briefly. Becquerel has shown that yellow phosphorus changes into the red modification, while mercuric chloride, in presence of oxalic acid, undergoes reduction. Hardy and Wilcocks have found that iodine is liberated from iodoform. The former has noticed that globulin is coagulated; Dryer and Hanssen state that, of a large number of proteins examined, only vitellin is coagulated. Jorissen and Ringer have shown that hydrogen and chlorine combine slowly; Berthelot has proved that iodine is liberated from iodic acid, though not to such an extent as from iodoform. Nitric acid is decomposed. Creighton and Mackenzie have just published an account of their research on hydriodic acid, in which they show that β rays increase the rate of decomposition. If the β rays are absorbed, the γ rays remaining produce a greater effect, but neither light nor β rays produces any effect in absence of oxygen.

The action of α and β rays on glass and precious stones with production of colour changes and phosphorescence has been dealt with sufficiently in Part I.

The Curies observed that oxygen is converted into ozone. The statement has been repeatedly confirmed. The action is probably to be attributed to α particles. So also is the production in small quantity of hydrogen peroxide in water containing emanation dissolved in it (Ramsay and Cameron).

Ramsay has noticed that dry oxygen in contact with mercury attacks it when emanation is present. Moist oxygen does not. The phenomenon is, no doubt, connected with the formation of ozone.

Ramsay and Cameron have just published a detailed account of the action of radium emanation on water and on certain gases. The apparatus was a modification of that shown in fig. 2; the capillary tube L was replaced by a measuring tube such as those shown in fig. 3, *a* and *b*.

The emanation, along with excess of hydrogen, was separated from the radium bromide solution as previously described. With the liquid or gas on which its action was to be tested, it was frozen solid at -185° in the bulb J, by means of liquid air; the excess of hydrogen was pumped away. An alternative method was to add the emanation directly to the pure gas under observation, along with hydrogen or oxygen, according as one or other of these was among the products of

the reaction. The apparatus was modified by the removal of the phosphorus pentoxide tube where water, or gases reacting with phosphorus pentoxide, were to be treated.

In all cases, by raising the reservoir H, the gases were forced through the apparatus, past the stopcock K, into a measuring tube, as in fig. 3, *a*. This contained a piece of blue glass, sealed to the tube, and broken off at a very fine point A, to which the mercury surface was accurately adjusted by raising or lowering the reservoir M (fig. 2). The difference in height between the mercury surface at A and that in the reservoir, subtracted from the barometric pressure, gave after the necessary temperature corrections the pressure exerted at 0° by the perfectly definite volume of gas enclosed. Readings of the

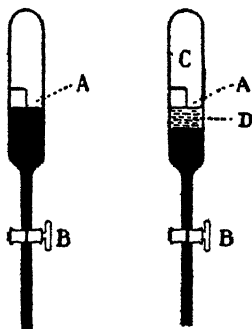


FIG. 3.

pressure were taken daily. In each the tap was opened, the mercury readjusted to the point A, and the tap then closed, so that the gas was kept at constant volume throughout the experiment. By calibrating the measuring tube with mercury at the end of the experiment, the capacity was found. The observed pressures then gave the actual volume of gas at 0° and 760 millimetres pressure, at each time of observation.

After three or four weeks, the amount of emanation remaining was practically negligible; by this time also the pressure became constant within the limit of error. When water and gas were both present the point A was set to the surface of the water as in fig. 3, *b*. In this way, while the tap B was closed, the gas and water phases, C and D, were both kept at constant volume. There is considerable reason to believe that emanation decomposes in different media at the same rate,

If it obeys Henry's Law, it will divide itself in a certain ratio between the gas and water phases, and this ratio will remain unaltered during the whole of the experiment. The emanation in each phase will produce its own effect. The difference in the two effects is the volume change measured.

As each experiment lasted a month, means were devised to subdivide the emanation and the gas containing it, and carry on several experiments simultaneously. In this way a number of interesting results were obtained. The qualitative results may be first summarised. Several experiments confirmed the rapid decomposition of water, and recombination of hydrogen and oxygen. Hydrochloric acid is decomposed into hydrogen and chlorine; mercury absorbed the chlorine, forming a deposit of mercurous chloride. Ammonia was decomposed into nitrogen and hydrogen, and these gases recombined; the amount of recombination (measured by absorbing the ammonia with calcium chloride) was smaller for the same quantity of emanation than of the decomposition.

In two experiments with carbon dioxide, there was a slight contraction of volume, due apparently to the action of oxygen—a product of the decomposition—on mercury, while a ring of carbon was deposited in appreciable quantity above the mercury surface. In a third experiment an attempt was made to absorb the oxygen with yellow phosphorus as fast as it was set free, but the phosphorus was immediately converted into the red variety. In this case, too, there was slight but steady increase in volume, and the gas finally contained a small quantity of oxygen mixed with a comparatively large quantity of carbon monoxide.

Starting with pure carbon monoxide, the pressure steadily decreased, and the deposit of carbon was appreciable. The gas at the end of the experiment contained both oxygen and carbon dioxide. It would therefore appear that with both oxides of carbon all possible reactions take place.

An extremely curious result was obtained with a mixture of steam and hydrogen jacketed at 130° . No decomposition (which would be indicated by an increase in pressure) was observed during six hours. This is the only experiment in which no change was observed; with water the change is immediately noticeable. Since hydrogen and oxygen combine appreciably at 130° , it can only be concluded that, if any

decomposition takes place, the rate of recombination must be sufficiently rapid to reunite any oxygen and hydrogen formed.

It has been shown in Part I. that all radioactive elements decompose according to the law expressed by the exponential equation

$$\frac{v_0}{v_t} = e^{-\lambda t} \quad (1),$$

where v_0 is the original amount (volume, if a gas) v_t that at time t , and λ is constant for the particular element (0.1795 for radium emanation if time is reckoned in days). It was also shown that,

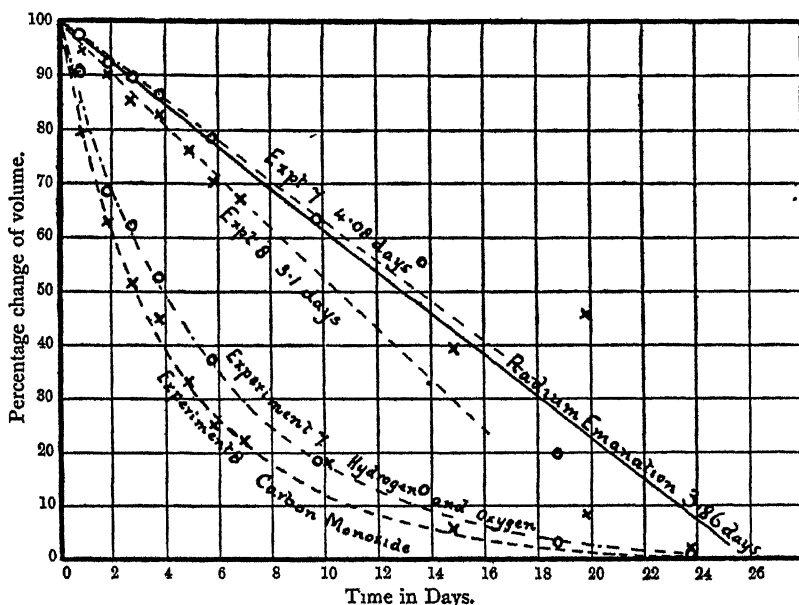


FIG. 4.

although all the elements have an infinite life, yet their half-life period is quite definite; and, as has already been mentioned in this paper, Sackur's value for the emanation is 3.86 days.

The chemical action induced by radium emanation obeys the same law. Thus, if V_0 is the initial volume of gas under observation, V_t the volume at time t , and V_∞ the final volume, then

$$\frac{V_0 - V_\infty}{V_t - V_\infty} \cdot \frac{v_0}{v_t} = e^{-\lambda t} \quad (2)$$

The curves in fig. 4 show the values for $(V_t - V_\infty)$ plotted against time, taking $(V_0 - V_\infty)$ as 100, and the corresponding logarithmic values together with the logarithmic curves for

radium emanation. These all give points lying on straight lines, and the curves closely resemble those for the decomposition of the emanation itself. These examples can be taken as typical.

The following table shows the period of half decay, and the values for λ . In the case of experiments 5—7, the measuring tube contained phosphorus pentoxide to absorb the water as fast as it was formed.

No. of Expt.	Reaction.	Total pressure change.	Volume change.	Half time.	λ .	Amount of Emanation.	V_E
1.	$2\text{H}_2\text{O} \rightleftharpoons 2\text{H}_2 + \text{O}_2$	459.5 mm.	0.766 c.c.	3.35 days	0.207	0.081 E	9.46 c.c.
2.	Do.	84.7 "	0.390 "	3.47 "	0.200	0.062 E	6.29 "
3.	$2\text{H}_2 + \text{O}_2 \rightleftharpoons 2\text{H}_2\text{O}$	232.5 "	0.668 "	3.40 "	0.204	0.093 E	7.18 "
4.	Do.	60.2 "	0.221 "	4.37 "	0.159	0.025 E	8.84 "
5.	$2\text{H}_2 + \text{O}_2 \rightleftharpoons 2\text{H}_2\text{O}$	84.3 "	0.555 "	4.08 "	0.170	0.044 E	12.61 "
6.	Do.	124.8 "	0.526 "	4.30 "	0.161	0.105 E	5.01 "
7.	Do.	229.6 "	0.476 "	4.08 "	0.170	0.063 E	7.56 "
8.	$\begin{cases} 2\text{CO} = 2\text{C} + \text{O}_2 \\ 2\text{CO} = \text{CO}_2 + \text{C} \end{cases}$	49.5 "	0.177 "	3.10 "	—	0.049 E	—
9.	$\text{N}_2 + 3\text{H}_2 \rightleftharpoons 2\text{NH}_3$	76.5 "	0.232 "	3.17 "	—	0.239 E	0.97 "
10.	$2\text{NH}_3 \rightleftharpoons \text{N}_2 + 3\text{H}_2$	76.9 "	0.427 "	(1.4) "	—	0.230 E	(1.86) "
11.	$2\text{HCl} \rightleftharpoons \text{H}_2 + \text{Cl}_2$	—	0.538 "	(5.9) "	—	0.069 E	(7.30) "

In most cases the total pressure change was small. A slight error of the final readings—due to leakage or any other cause—materially affects the half time as determined from the curve. In view of these facts the truth of equation (2) must be held as established.

The authors point out that it follows from this that the amount of chemical action taking place in any time is strictly proportional to the amount of emanation which decays in that time. From this it can be deduced that other conditions being the same, *each atom of emanation, as it disintegrates, produces a certain definite chemical effect.*

In the seventh column are given the amounts of emanation used in each experiment, calculated on the assumption that E is the quantity of emanation obtained from one gram of radium (as metal) dissolved in water, in 3.86 days. The eighth column shows the total volume change which E c.c. of emanation would effect. In experiments 5—7 the conditions were similar; the effect of E varies from 5.01—12.61 c.c. gas combined. It follows therefore that some other factor besides the amount of emanation present determines the total amount of chemical action. In all probability there is a surface effect; but the present data do not allow its determination. Since in any single experiment

the surface remained constant throughout the experiment, it did not affect the time of half change, but it prevents the determination of the actual volume of gas decomposed in the water phase in the first two experiments. In both, the water phase was much less in volume than the gas phase, the action on water appears to be much greater, volume for volume, than on the mixture of gases.

The two final experiments show considerable differences from the theoretical values. In the case of ammonia these were due to traces of air, admixed with ammonia, with resulting production of water, which immediately saturated itself with the gas. In the final experiment both volume and pressure were read, and the volume was not kept constant; the error in reading was therefore much greater than usual.

The mechanism of the reactions is probably the same that produces the ions which discharge an electroscope (Part I.). Collision between the α particles shot off from the emanation and the gas-molecules produces charged ions of the gases, and these combine. With hydrogen and oxygen, for instance, water is formed, and the resulting diminution in volume, and consequently in pressure, is measurable. With water the actual collisions produce atoms of hydrogen and oxygen, some of which give molecules—as in ordinary electrolysis.

The final part of this paper will contain accounts of the chemical and spectroscopic evidence of the transformation of elements, together with the recent attempts which have been made to cause transmutation.

The chief authorities which have been consulted in preparing Parts I. and II. of this paper are :

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RECENT WORK ON THE CHEMISTRY OF CHOLESTERINE

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CHOLESTERINE, or Cholesterol, is a white crystalline substance which appears to be found in most of the tissues and fluids of animals, and—in the isomeric form of phytosterine or vegetable cholesteroline—in the seeds and shoots of plants. The human brain contains about 27 grammes, or $2\frac{1}{4}$ per cent. of its weight of cholesteroline (Benecke),¹ and it is found in smaller quantities in the spleen, in the blood ($\cdot 1$ per cent.), in milk ($\cdot 04$ per cent.), and in bile ($\cdot 2$ – $\cdot 4$ per cent.). It occurs also in certain tumours and other pathological products, and particularly in gall-stones, which, containing as they do 90–98 per cent. of cholesteroline, constitute the chief source of the substance for scientific purposes. It is only necessary to dissolve the powdered stones in benzene, allow the solution to deposit a small quantity of pigment, filter, and nearly pure cholesteroline separates out.

From the brain it is best obtained by the method of Rosenheim.² The minced material is mixed with some sand and three times its weight of plaster of Paris. This combines with the water, and the whole mass sets hard in a few hours, and may be broken up to a coarse powder very suitable for the extraction, which is done with acetone. Lecithin and kephalin being insoluble in cold acetone, the extract obtained, on evaporation, yields nearly pure cholesteroline. From other organs and tissues it is usually obtained by extracting the dried substance with ether. The separation of the fat is stated to be frequently incomplete even after prolonged extraction, and in this connection it may be mentioned that C. Dormeyer³ has shown that by submitting the material to artificial digestion a further yield

¹ Cf. also Miss M. C. Tebb, *Journ. Physiology*, **34**, 107.

² *Journ. Physiology*, **34**, 104.

³ Pflug. *Archiv.* **61**, 341.

(75 per cent.) of fats may be obtained. The fatty extract is saponified with alcoholic caustic soda, or with sodium ethylate in etherial solution (Kossel and Obermüller). In the latter case saponification is rapid and complete in the cold, and the separation of the soaps is easy. The cholesterine thus obtained crystallises from 85 per cent. alcohol in thin rhombic plates containing a molecule of water of crystallisation, or from ether, chloroform, etc., in anhydrous silky needles melting at 147° . In the animal kingdom this widely diffused cholesterine is the only body of its type hitherto discovered, with the exception of the Iso-cholesterine of melting-point 138° which, according to Schulze,¹ is contained, along with cholesterine, in wool-fat and must apparently be regarded as a cholesterine excreted through the skin of the sheep. But in the vegetable world the labours of many investigators² have brought to light a large number of vegetable cholesterines which are very similar in properties to cholesterine itself. In the work of von Hauth² we find references to forty-eight such bodies obtained from the most varied plants, their seeds, leaves, and juices, and from vegetable oils and waxes. No fewer than thirty-one of these, however, are isomeric with cholesterine and have melting-points lying between 130° and 137° C., and recent work makes it extremely probable that they are all more or less impure forms of one substance, phytosterine. Burián³ in 1897 for the first time made a careful examination of a phytosterine extracted from germinating wheat, which he called sitosterine. He purified it with great care and obtained it back unaltered from pure derivatives, and showed that its chemical properties formed a complete parallel with those of cholesterine. Thus it forms crystalline esters, a dibromide, and by loss of water a hydrocarbon, as cholesterine itself. Windaus and Hauth,³ again, in 1907 made a study of the original phytosterine of Hesse obtained from Calabar beans. They at once recognised that Hesse's product was a mixture, and after much difficulty succeeded in separating it into two bodies: (1) stigmasterine, an alcohol $C_{30}H_{48}O$, melting-point 170° , which forms a characteristic tetrabromide; and (2) a phytosterine, present to the extent of some 75 per cent. of the mixture, which proved identical with

¹ *Ber.* 6, 1075 (1873).

² For references see von Hauth, *Inaug. Diss.* Freiburg, 1907.

³ See bibliography at the end of this article.

the sitosterine of Burián. With this discovery in mind, a glance at the tables given by von Hauth shows that most of the phytosterines that have been described with melting-points of 130° to 137° must be phytosterine mixed with some impurity. We may say, then, that the higher plants contain as their typical cholesterol constituent, phytosterine, an unsaturated alcohol, isomeric with cholesterol, of melting-point 137° .

Its empirical formula is difficult to settle exactly by analysis. The results of Chevreul, Couerbe, Marchand, and others pointed to the proportions $C_{27}H_{46}O$; Gerhardt, however, suggested $C_{26}H_{44}O$, and for many years this was adopted. Obermüller¹ attempted to decide the question by accurate analysis of pure derivatives, and his work again seemed to confirm the formula $C_{27}H_{46}O$; but in 1894 Mauthner and Suida, on reviewing all the older analyses, pointed out that the carbon values in particular were always higher than would be required for this formula, and that they corresponded very much better with a formula $C_{27}H_{44}O$. Further, such an assumption would remove difficulties they had found in assigning formulæ to the hydrocarbons derived from cholesterol by the loss of the elements of water. They accordingly analysed carefully dried and purified cholesterol and its compounds, and in every case the results appeared to confirm their conclusion. For instance, for cholesterol they found :

	I.	II.	III.	IV.	V.	$C_{27}H_{46}O$	$C_{27}H_{44}O$
C. .	84.07	84.04	84.00	84.12	84.02	83.96	84.37
H. .	12.24	—	11.79	11.85	11.84	11.92	11.46
Dried at	110°	130°	115°	125° — 130°			

and when it is remembered that the carbon values in organic analysis are usually lower than those required by theory, it will be seen that there is good reason for the adoption of the lower hydrogen formula. All the modern workers on the subject, however, still use $C_{27}H_{46}O$ as the formula for cholesterol; and to avoid confusion, therefore, it will be employed in the present paper.

Chemically cholesterol is an unsaturated secondary alcohol. It combines with two atoms of bromine, forming the dibromide,

¹ *Zeit. Physiol. Ch.* 15, 37.

$C_{27}H_{46}O \cdot Br_2$, and thus contains one double link in the molecule. The hydroxyl of the secondary group $CH \cdot OH$ can be replaced by chlorine, giving cholesteryl chloride, $C_{27}H_{45}Cl$, and it reacts readily with acids forming esters, of which the olcate and palmitate occur in blood serum. Besides these the formyl, acetyl, propionyl, and benzoyl derivatives are readily prepared, the reaction with acetic anhydride in particular being so smooth that it has been employed for the quantitative estimation of cholesterine. From what follows it will be seen that the hydroxyl group appears to be situated in a ring and the double link at the end of an open chain of three or four carbon atoms, so that the double link is in the $\delta\epsilon$, or $\epsilon\zeta$, position to the OH group. Beyond this but little is known of the structure of the cholesterine molecule. The opinion is gaining ground that the body is related to the higher terpenes¹ and the resin acids, and that it contains probably four reduced rings, being based upon a reduced hydrocarbon, just as abietic acid is now recognised as coming from a reduced methyl isopropyl phenanthrene (Retene).² But our knowledge of the structure of the complex terpenes is so slight that even if cholesterine proved to be related to them a great deal of work would be required before we should be in a position to apply their properties and reactions to the solution of the cholesterine problem. An experimental difficulty, too, lies in the great stability of cholesterine. While it readily yields derivatives containing the same number of carbon atoms in the molecule, it can only be broken down by somewhat drastic methods, and the process is accompanied by an enormous wastage of a material which, probably, will always be scarce. Mauthner and Suida, for example,³ who have made destructive experiments of this kind, obtained a hydrocarbon, $C_{16}H_{26}$, which, on chlorination and subsequent treatment, gave two bodies—one soluble, the other insoluble, in ether. From 162 grammes of the former, after distillation with lime, only '3 gramme of a body smelling like menthol was obtained, barely enough for a melting point and a single combustion. They have since been engaged upon oxidation experiments which seem more hopeful of results.

¹ Tschirch and Studer, *Archiv d. Phar.* **241**, 542; Mauthner and Suida, *Monatshefte für Chemie*, **15**, 114; **24**, 173.

² *Ber.* **36**, 4200; *Proc. C. S.* **20**, 112.

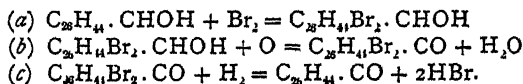
³ *Monatshefte für Chemie*, **24**, 648.

The work of the last few years may conveniently be considered under the following heads:

- (1) The ketone cholestenone.¹
- (2) Oxidation products.
- (3) Reduced cholesterol.

CHOLESTENONE

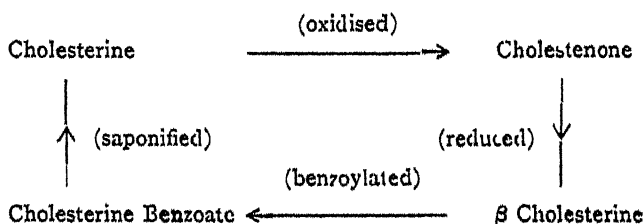
The conversion of cholesterol to the corresponding ketone proved difficult, as with most oxidising agents the action proceeds too far. Diels and Abderhalden, however, in 1904, succeeded in preparing cholestenone by using copper oxide at a temperature of 280°–300°. The finely powdered oxide is added gradually to the melted cholesterol, and the black, tough mass resulting dissolved in cold methyl alcohol. The solution on standing deposits beautiful needles of the ketone, which melt at 81°–82°. The yield, as might be expected, is somewhat low, being only 25–30 per cent. of the theoretical, and the high temperature necessary for the reaction renders it uncertain whether any other changes take place within the molecule beside the simple oxidation. Windaus (1906) showed that the ketone could be prepared under conditions which rendered any such molecular alterations far less likely. The cholesterol is converted to the dibromide, and the double link, being thus “protected,” the oxidation can be carried out, either with chromic acid at 70°, or with 4 per cent. acid permanganate in the cold. The dibromcholestenone formed as an intermediate product on treatment with zinc dust and acetic acid leaves the ketone. Yield 60 per cent of the theory. Thus:



The resulting body was shown to be identical with that of Diels and Abderhalden, a very interesting result since it goes to prove that cholesterol is not altered structurally by oxidation with copper oxide at a temperature of 300°.

¹ It might be mentioned that the ending “-ol” indicates an alcohol; “-one” a ketone. The syllable “-ene” indicates an unsaturated, “-ane” a saturated body, Cholesterol is thus more correct than cholesterol, but the latter name has long usage to justify it.

The discovery of this ketone supplies us with a fresh starting-point from which to carry out an experimental attack on cholesterine. Instead of the comparatively inactive secondary group CHOH we now have the highly reactive carbonyl, and further the properties of unsaturated ketones have already been carefully investigated,¹ so that it should be possible at once to draw accurate conclusions from the experimental data. It was, until recently, regarded as doubtful whether the conversion of cholesterine to cholestenone was as simple as represented above, or whether some intramolecular change might not after all have taken place. But that the "carbon skeleton" of these bodies is the same, is rendered very probable by the recent work of Diels and Linn² (1908). By the action of sodium in boiling ethyl alcohol, they were able to reduce cholestenone to an (isomeric) β cholesterine of melting-point 160° . This substance they regard as a *cis-trans* modification of cholesterine, since it gives the same dibromide and esters as cholesterine itself. They also obtained β cholesterine by heating cholesterine to 300°C. with a trace of iron salt (*loc. cit.*). In this reaction cholestenone was also produced—a very curious result. Their conversion of cholestenone to cholesterine is shown in the following scheme:



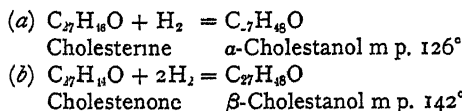
The relative position of the CO group and the double link in cholestenone has given rise to some discussion. Diels and Abderhalden (1906) considered the double link to be in the $\alpha\beta$ position to the carbonyl. They found that cholestenone-oxime formed an additive compound with hydroxylamine, a known property of $\alpha\beta$ unsaturated ketones, and that further,³

¹ Especially by Harries and his students.—*Annalen*, 330, 185, and many previous papers.

² *Ber.* 41, 260.

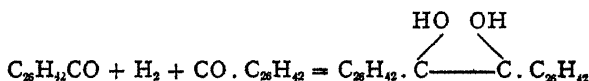
³ *Ber.* 39, 884.

on reduction with sodium in amyl alcohol solution, both cholesterol and cholestenone reacted thus :

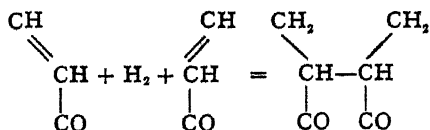


Such a reduction only takes place with $\alpha\beta$ unsaturated ketones, but Windaus (1907) has shown that this is probably not a case of reduction at all, since α -cholestanol can be obtained by the action of sodium amylate, and that it does not contain two atoms of hydrogen more than cholesterol, but, although saturated, is isomeric with that body. He concludes that it is formed by a conversion of the open chain of cholesterol to the cyclic form. This, however, is not accepted by Diels and Linn, whose work above-mentioned proves that cholestenone can be reduced. They showed also that the β cholesteroline obtained, when treated with sodium and amyl alcohol, gives α -cholestanol. They consider that sodium amylate possesses reducing properties, since cholestenone when treated with it,¹ gives not β , but α -cholestanol.

In 1906 Windaus proved that cholestenone, when reduced by sodium amalgam, gives an unsaturated pinacone $\text{C}_{64}\text{H}_{96}$ or 90O_2 thus :



According to Harries² all unsaturated ketones, except those with the double link in the $\alpha\beta$ position, react in this way, giving a pinacone. Those unsaturated in the $\alpha\beta$ position give a saturated dimolecular diketone :

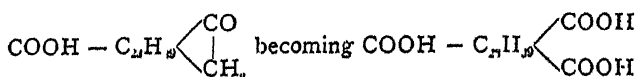


Supposing next that the double link might be in the $\beta\gamma$ position, Windaus heated the ketone for a long time with 3 per cent. alcoholic hydrochloric acid, with 10 per cent. alcoholic potash, and finally with diethylamine at 160° . These reagents, which

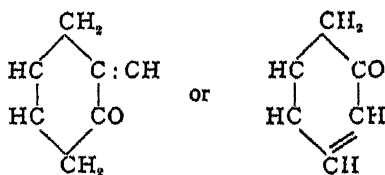
¹ *Ber.* 41, 554.

² *Ann. der Ch.* 330, 213.

might by analogy be expected to shift a double bond from the $\beta\gamma$ to the $\alpha\beta$ position,¹ were without effect on cholestenone. It seems then that the $\beta\gamma$ position is also excluded. Realising that if intramolecular change takes place during these experiments the conclusions derived from them are rendered unreliable, Windaus (1906) oxidised the ketone with neutral permanganate which, according to Tiemann and Semmler,² is not likely to produce such changes. He found that the chief product of the reaction was a saturated keto-monocarboxylic acid $C_{26}H_{42}O_3$, containing one carbon atom less than the parent substance, and this acid, on further oxidation, passes to a tricarboxylic acid $C_{26}H_{42}O_6$,



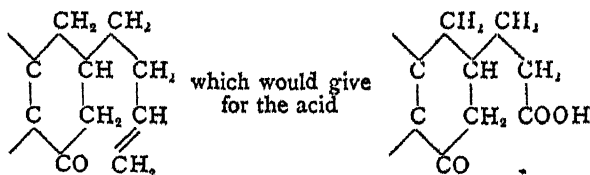
"This formation of a keto-acid must negative the explanation of cholestenone as an $\alpha\beta$ unsaturated ketone. In that case it appears impossible to construct a scheme whereby an $\alpha\beta$ unsaturated ring system such as



passes to a saturated keto-monocarboxylic acid with CO bound in the ring and poorer in carbon atoms." As the simplest explanation he suggests that in cholestenone there is an end group $\text{CH} = \text{CH}_2$, which oxidises thus :



Combining this with other evidence to be mentioned below, he provisionally writes Cholestenone



the double link being in the $\epsilon\zeta$ position to the carbonyl.

¹ Blaise, *C. R.*, 138, 636.

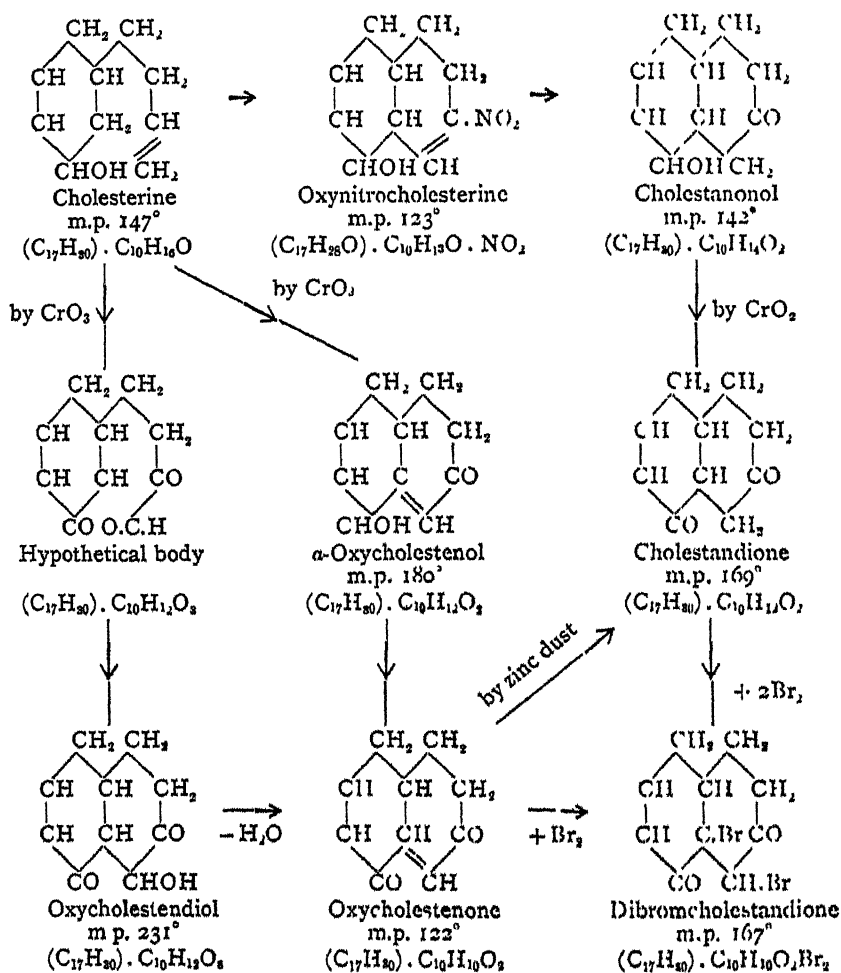
² "On Pinene," *Ber.* 38, 1345.

THE OXIDATION OF CHOLESTERINE

With chromic acid, permanganate, etc., under ordinary conditions, no breaking down of the cholesterol molecule takes place, but various neutral and acid bodies are formed still containing the same number of carbon atoms as the original substance. Mauthner and Suida accordingly (1903) investigated the action of strong nitric acid and of concentrated permanganate solution both in the cold and at 100°. In each case they obtained among other products two homologous acids $C_{13}H_{18}O_8$ and $C_{13}H_{16}O_8$, of which the last appears to be the end product of the action, since further oxidation led to its recovery unchanged. They are both tetrabasic. Their calcium salts contain eight molecules of water of crystallisation, and though soluble in cold water are thrown out of solution on heating. By the loss of a molecule of water or carbon dioxide the acids yield new compounds which, according to Mauthner and Suida, would greatly simplify the work on cholesterol. The authors, however, were not able to connect these products with any known substances, and so far as we know, have not continued their investigations on the subject. Windaus also in 1903 found that by prolonged boiling with highly concentrated nitric acid acetic acid was formed in some quantity, together with a small amount of dinitro-isopropane $(CH_3)_2 \cdot C \cdot (NO_2)_2$ —results which led him to infer the presence of the methyl and isopropyl groups in cholesterol.

The above results represent, we believe, the only resolutions of the cholesterol molecule so far achieved. Ordinary oxidising agents give rise to a number of twenty-seven carbon derivatives, important from a chemical standpoint, but whose study is somewhat difficult as they are described in the literature under varying names, and their empirical formulæ, differing only by one or two atoms of oxygen and hydrogen, give no clue to their connection with cholesterol and with one another. The following structural formulæ (after Windaus), even if they are not accurate representations of the molecular portions considered, will be found most useful in bringing out these relations. It will be noticed that chromic acid and permanganate seem to attack first the unsaturated side chain of the cholesterol, which then undergoes condensation with the production of a new ring, and that, starting from cholesterol,

it is possible to arrive at the body dibromcholestandione by three different paths :



The oxidation of cholesterol with chromic acid gives rise to three neutral products— α -oxycholestenol, oxycholestenone, and oxycholestendiol. Of these the first, $C_{27}H_{42}O_2$, is a substance apparently unsaturated and containing a $CHOH$ group which is very easily oxidised to carbonyl, giving oxycholestenone, $C_{27}H_{40}O_2$. This ketone is certainly unsaturated, yielding with bromine a crystalline dibromide, but the function of the second oxygen atom in both these bodies was for long entirely unknown.

Recent observations show that oxycholestenone forms an ethyl ester which speaks for the presence of hydroxyl, but that on attempting to reduce either the ketone or the ester, instead of the expected replacement of the tertiary hydroxyl by hydrogen,¹ two hydrogen atoms are added smoothly at the double link and cholestandione is formed quantitatively. In the case of the ester, therefore, reduction and saponification take place at the same time, and the ester is also saponified merely by heating with acetic acid. On account of these peculiarities, Windaus concluded that oxycholestenone contains a keto-group which functions in the enol form. This view is strengthened by the close relationship in which it stands to cholestandione, for whose formula there is other evidence.

Oxycholestendiol, $C_{27}H_{42}O_3$, the third body produced by the action of chromic acid, owing to its insolubility in benzene, is easily separated from the others. The attack of the oxidiser in this case seems to run on different lines, and to explain it we have to assume the formation of the intermediate aldehyde body shown in the scheme. Oxycholestendiol is saturated to bromine, but it forms no hydrazone, so that direct experimental evidence for the presence of the carbonyl group is lacking. Treated with reagents like alcoholic potash, sulphuric or hydrochloric acids, it loses water and passes to oxycholestenone. This change takes place with the utmost readiness, and constitutes, perhaps, the most characteristic property of the body. It necessitates the existence of a hydroxyl group, though, on attempting to acetylate this, loss of water occurs and oxycholestenone is again produced. The formula given is therefore based chiefly upon this obvious connection with oxycholestenone.

The destructive effect of hot concentrated nitric acid on cholesterol has already been mentioned. But in cold glacial acetic acid solution cholesterol can be nitrated, a crystalline substance, $C_{27}H_{42}N_2O_6$, which melts without decomposition at 128° , being obtained. This body, which has been called oxynitrocholesteryl nitrate, is singularly stable towards most chemical agents—acetyl oxide, phenyl hydrazine, bromine, alkalies, permanganate, and chromic acid being without effect. That it is an ester of nitric acid was proved by preparing oxynitrocholesteryl acetate from cholesteryl acetate, saponifying this and combining the oxynitrocholesterine with nitric acid again ;

¹ Semmler, *Ber.* 37, 2520.

but the other nitrogen atom does not appear to be in a true nitro group, since on reduction ammonia is lost and a saturated keto-alcohol, $C_{27}H_{44}O_2$, is formed. This behaviour recalls the observations of Wallach and others on α and β phellandrene, anethol, etc., which they showed could be converted, through nitro-derivatives, into saturated ketones or aldehydes. In the phellandrene compound, for example, Wallach assumes the grouping

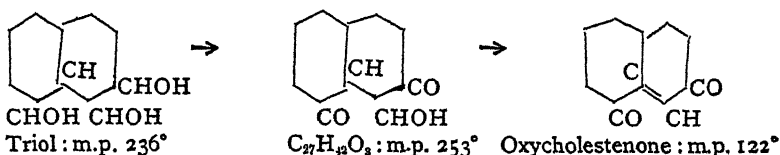


and however this may be, the effect of reduction is certainly to transform the group $\text{CH}:\text{CH}$ into the group $\text{CO}.\text{CH}_2$, and we may perhaps assume the same change with the cholesterine derivative.

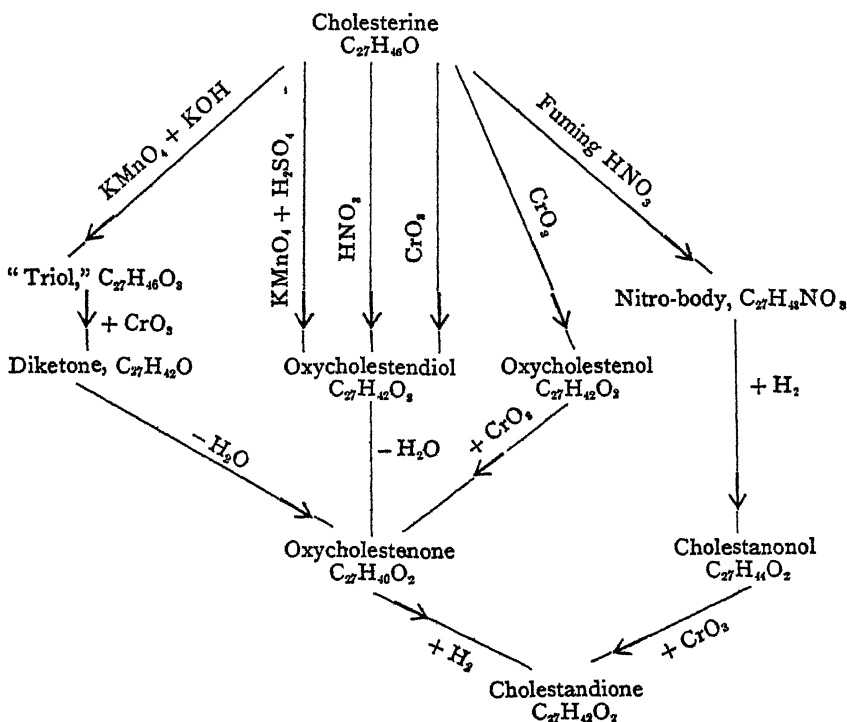
The keto-alcohol cholestanonol, therefore, probably contains $\text{CO}.\text{CH}_2$ instead of the $\text{CH}:\text{CH}$ of the parent substance. It is a white crystalline body melting at $142-143^\circ$. The keto-group is easily characterised by the formation of a crystalline nitrophenyl-hydrazone and the hydroxyl by the corresponding acetyl and benzoyl derivatives. This hydroxyl is easily attacked by most oxidising agents, and a diketone, cholestandione, $C_{27}H_{42}O_2$, is produced. This very important body, which, as mentioned above, is also prepared by the reduction of oxycholestenone, melts at 169° and gives a well-defined dioxime. Bromine acts as a substituent, giving a dibromcholestandione, $C_{27}H_{40}O_2\text{Br}_2$, which in melting-point and other properties proves identical with the dibrom-addition product of oxycholestenone. The close connection between the two compounds is thus emphasised, and a valuable connecting link given us from which conclusions may be drawn as to formulæ. Since the two bromine atoms are added at the double link of oxycholestenone they should be in the 1:2 position to each other. Again, if the bromination of cholestandione follows the usual course with such ketones, each should be in the α β position to a carbonyl group, giving us the grouping $\text{CO}.\text{CH}_2.\text{CH}_2.\text{CO}$, and making cholestandione a γ -diketone. This view is to some extent confirmed by its behaviour towards hydrazine and its oxidation. Treated energetically with chromic acid it gives cholestanone dicarboxylic acid, which still contains one keto-group. If cholestandione were a β -diketone the acid would be a β -ketonic acid; but it is stable on heating to 200° with potash, which is all against

this assumption, and the γ position remains therefore as most probable.

Towards neutral permanganate cholesteroline is very stable, but on the addition of alkali a triatomic alcohol, $C_{27}H_{46}O_3$, is obtained, though the yield is only some 5 per cent. of the theoretical. This "triol" is readily attacked by chromic acid, giving a diketone isomeric with oxycholestendiol, which like that body easily passes to oxycholestenone with loss of water. Thus :

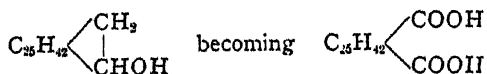


These various oxidation effects are put together in the following scheme :

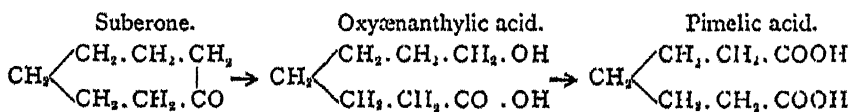


In addition to the neutral bodies described above, chromic

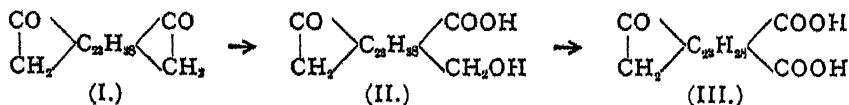
acid, acting on cholesterine, gives a small proportion of acid bodies. The chief of these has the formula $C_{27}H_{41}O_4$, and had previously been prepared by Diels and Abderhalden by the action of alkaline hypobromite. It is dibasic, giving on titration with soda an acid, with potash a neutral salt, while by direct esterification the monomethyl ester is obtained. Its formation from cholesterine is simple:



Chromic acid also attacks cholestanonol and cholestandione, giving in each case a well-defined body, cholestanone dicarboxylic acid, $C_{27}H_{43}O_5$. This melts at 218° , and, being difficultly soluble in benzene, is easily crystallised. It forms a highly characteristic methyl ester melting at 113° , and the presence of the CO group is shown by the formation of an oxime and a semicarbazone. The formula given to it assumed that the $CO \cdot CH_2$ group of cholestanonol is opened and replaced by the two acidic groups COOH. In order to verify this Windaus made use of the observation of Baeyer and Villiger¹ that ammonium persulphate acts on cyclic ketones, changing them to oxyacids or their lactones. For example, suberone gives oxyænanthyllic acid thus:



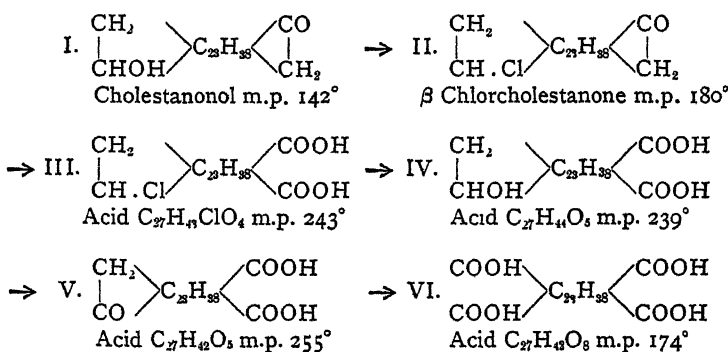
Cholestandione (I.) behaves analogously. With persulphate it gives a monobasic acid $C_{27}H_{41}O_4$ (II.), and this, oxidised with chromic acid, yields cholestanone dicarboxylic acid (III.).



Using cholestanonol as a starting-point, some interesting acids were obtained by Windaus and Stein. The numerous difficult transformations, which were carried out with great

¹ *Ber.* 32, 3625, etc.

skill, are best followed with the following formulæ taken from their original paper (1904):



The hydroxyl of (I.) was replaced by chlorine through the action of phosphorus pentachloride, and was thus "protected" from the oxidising agent employed—viz. nitric acid in glacial acetic acid solution. The acid (III.), $\text{C}_{27}\text{H}_{43}\text{ClO}_4$, is obviously formed by the opening of the $\text{CO} \cdot \text{CH}_2$ group of cholestanonol. The chlorine was now replaced by hydroxyl, giving the oxy-acid (IV.), which was converted by dilute chromic acid mixture to the keto-acid (V.). This forms an oxime melting at 213° . (V.), treated with strong chromic acid, gave the tetrabasic acid (VI.). In this way two of the rings of the original cholestanonol were opened, and it is possible that the results may be of the greatest value in clearing up the constitution of cholesterol.

COPROSTERINE

As cholesterol is so widely and abundantly diffused throughout the organs and tissues, it becomes of interest to discover whether it appears among the waste products of the human body. Bondzynski and Humnicki, in 1896, showed that it was present in the fæces as a hydrogenated derivative, to which they assigned the formula $\text{C}_{27}\text{H}_{48}\text{O}$. This coprosterine, as it was called, is excreted at the rate of about $\frac{3}{4}$ of a gramme per day. So far as it has been examined it functions as a secondary alcohol, though, unlike cholesterol, it is saturated to bromine. It crystallises from alcohol in masses of fine needles melting at $99\text{--}100^\circ \text{C}$., has a rotation $[\alpha_D]$ of $+24^\circ$, and forms crystalline acetyl, benzoyl, cinnamoyl, etc., derivatives.

Bondzynski and Humnicki regard coprosterine as a dihydro-

derivative of cholesterine, but all attempts to prepare it from that body by chemical or bacterial means have, up to now, been unsuccessful. They proved that it was never found in any of the organs, tissues or fluids of the body, and therefore concluded that the reduction took place in the intestines. A man was fed with cholesterine, and was found to excrete it mainly as coprosterine, and following up this observation Muller examined the effect of a milk diet, in which putrefactive changes in the intestine are reduced to a minimum. He found that the fæces of both the suckling and the milk-fed adult contained cholesterine only and no coprosterine, while on any other diet coprosterine only was observed. The results seem to confirm the hypothesis of a bacterial formation in the intestines. The authors also have noticed that the dog, when given various ordinary diets such as meal or cooked meat, normally excretes a small quantity of cholesterine. When fed on raw brain, however, the extract of the fæces yields coprosterine only and no cholesterine.

This résumé of recent work shows that while some progress has been made in our knowledge of the structural chemistry of cholesterine, a very great deal remains to be done. Of the physiological significance of this important body practically nothing is known. The probability is that the chemical work will follow the same course as that which resulted in the clearing up of the constitution of the terpenes, and that then, aided by a great advance in our knowledge of other physiological bodies and of metabolism in general, the problem of the origin and function of cholesterine in the system will be solved.

The following is a list of the most important recent papers on cholesterine, with an indication of their contents:

J. MAUTHNER and W. SUIDA—*Monatshefte für Chemie*:

- I.—xv. 85 (1894), Hydrocarbons; Chlorine and other Derivatives.
- II.—xv. 362 (1894), The Formula of Cholesterine.
- III.—xvii. 29 (1896), Hydrocarbons; Distillation of Cholesteryl Chloride.
- IV.—xvii. 579 (1896), Oxidation of Cholesterine; Oxycholestenol, etc.
- V.—xxiv. 175 (1903), Acids $C_{18}H_{34}O_8$ and $C_{18}H_{32}O_8$, etc.
- VI.—xxiv. 648 (1903), Nitrous Acid on; Hydrocarbon $C_{19}H_{38}$, etc.

J. MAUTHNER—*Ibid*:

- I.—xxvii. 305 (1906), Addition of Hydrogen Chloride.
- II.—xxvii. 305 (1906), Specific Rotations of Cholestene and Cholestane Derivatives.
- III.—xxviii. 1113 (1907), On Cholestene.

A. WINDAUS—*Habilitationsschrift*, Freiburg (1903).

— *Ber. der deut. chem. Gesellschaft* :

I.—**36**, 3752 (1903), Nitro-cholesterine, etc.

II.—**37**, 2027 (1904), Oxidation of Cholestandione.

III.—**37**, 3699 (1904), Acids from Cholesterine (with Stein).

IV.—**37**, 4753 (1904), Isomeric Acids from Cholestanone.

V.—**39**, 518 (1906), Preparation and Properties of Cholestenone.

VI.—**39**, 2008 (1906), Oxidation of Cholestenone.

VII.—**39**, 2249 (1906), Oxidation of Cholesterine.

VIII.—**40**, 257 (1907), Conversion to a "Triol."

IX.—**40**, 2637 (1907), Cyclocholesterine.

X.—**41**, 611 (1908), Oxidation of the Acid $C_{27}H_{44}O_4$.

WINDAUS and HAUTH, *Ber.* **39**, 4378 (1906), On Stigmasterine.

— — — *Ber.* **40**, 3681 (1907), On Phytosterine.

O. DIELS and E. ABDERHALDEN :

I.—*Ber.* **36**, 3177 (1903), Oxidation with Hypobromite.

II.—*Ber.* **37**, 3092 (1904), Cholestenone.

III.—*Ber.* **39**, 884 (1906), Reduction of Cholesterine and Cholestenone.

IV.—*Ber.* **39**, 1371 (1906), Reduction of Cholesterine and Cholestenone.

V.—*Ber.* **41**, 260 (1908), Reduction of Cholestenone.

VI.—*Ber.* **41**, 554 (1908), α and β Cholestane.

BONDZYNSKI, *Ber.* **29**, 477 (1896), Coprosterine.

BONDZYNSKI and HUMNICKI, *Zeit. Physiol. Ch.* **22**, 396, Coprosterine.

P. MÜLLER, *Zeit. Physiol. Ch.* xxix. 129 (1900), Excretion of Cholesterine.

DARMSTÄDTER and J. LIFSCHÜTZ, *Ber.* **31**, 97, 1122 (1898), Woolfat [Part v. and vi.].

J. LIFSCHÜTZ, *Zeit. Physiol. Ch.* **50**, 436 (1907), Oxidation of Cholesterine.

R. BURIÁN, *Monatshefte für Chemie*, xviii. 551 (1897), On Sitosterol.

THE BIO-CHEMISTRY OF ANIMALS AND PLANTS

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TABLE OF CONTENTS

	PAGE
THE NITROGENOUS CYCLE VOL. II.	676
Nitrification, Denitrification, N-Fixation.	
THE SYNTHESIS OF PROTEINS	681
In Moulds, Yeasts, Higher Plants and Animals.	
THE VEGETABLE PROTEINS	687
History, Occurrence and Properties, Constitution, Classification.	
THE SIGNIFICANCE OF PROTEINS IN THE PLANT. VOL. III.	106
Alkaloids, Indol-derivatives, Glucosides.	
PHYTOTOXINS, TOXOLECITHIDES	109
LIPOIDS AND THEIR BIOLOGICAL IMPORTANCE	112
INOSITE	117
ORGANIC PHOSPHORIC ACIDS, PHYTIN, INOSIC ACID, ETC.	118
CHLOROPHYLL AND ITS RELATION TO HÆMOGLOBIN	120

PART II

The Significance of Proteins in Plants.—In the present state of our knowledge of protein metabolism in plants it is difficult to form a definite opinion as to the significance of nitrogen in the vegetable world. So much is certain: that this element does not play the same rôle as in the animal body. In animals protein, in addition to the part it plays in repairing tissue-waste, serves as a source of energy. In certain lower plant organisms, such as the bacteria which act as nitrifying, denitrifying, or ureolytic agents, the necessary energy is doubtless furnished by nitrogenous substances, but this is quite exceptional. The place of the nitrogenous materials in the higher plants is taken by carbohydrates and fats, and the importance of nitrogen seems to consist in its use as a building material for the "organo-protein" in Voit's sense of the term, as opposed to "circulating protein." Expressing this statement, according to Voit's view, one might say that the "circulating protein," in relation to "organo-protein," plays quantitatively a greater rôle

in the animal than in the plant. In accordance with this view, we find in animals a much more pronounced nitrogenous katabolism than in plants; the healthy animal organism is in a state of nitrogenous equilibrium—*i.e.* approximately the same amount of nitrogen is excreted in the urine and fæces as is assimilated from the food. In plants this nitrogenous equilibrium does not exist, but the nitrogen contents of the organism grow steadily without nitrogen being lost in any other way than by the decay of older parts.

Final Products of N-Katabolism.—The question arises whether the plant ever completely breaks down proteins in the manner they are broken down in the animal body. The two common end-products of nitrogenous katabolism in animals, urea and uric acid, have certainly never been found in the vegetable world. E. Fischer has, however, isolated the mother substance of uric acid and other materials closely related to it; this he termed *purine*, and uric acid is an oxygenated derivative of this body.¹ It is therefore interesting to note that other purine derivatives occur in certain plants, and occasionally in quite large quantities, and are possibly end-products of metabolism. Of these *caffeine* (trimethyl-dioxy-purine) and *theobromine* (dimethyl-dioxy-purine) are the most important. The purine derivatives are especially abundant in those parts of the plant which are rich in protein, and in which metabolism is most active (*e.g.* in parenchymatous tissue, as in buds, young leaves, etc.). There is, therefore, some justification for assuming that they are formed from the proteins. It must be remembered, however, that we have so far no definite proof of this, and that they may just as likely be derived from the nucleins. E. Fischer's discovery of the pyrrolidine compounds (proline and oxyproline) among the cleavage products of proteins is suggestive as indicating a relationship of the latter to the *alkaloids*, in many of which (nicotine, cocaine, hygrine, atropine, hyoscyamine) the pyrrolidine ring occurs in connection with the pyridine ring. Still another cleavage product of proteins, namely tryptophane or indole-amino-propionic acid (isolated by Hopkins and Cole and recently synthesised by Ellinger), helps us to explain the relationship to proteins of another class of substances found as end-products in both the animal and plant organism—the *indole derivatives*. Tryptophane is

¹ See E. Fischer, *Untersuchungen in der Puringruppe* (1882—1906).

decomposed by putrefying bacteria into indole and scatole, amongst other substances. Indole is a frequent end-product of bacterial growth in protein material, but it is also found in phanicrogamic plants. It occurs, for instance, to the extent of 2·5 per cent. in the essential oil of jasmine. This oil also contains anthranilic acid, a substance which is obtainable from indigo, another derivative of indole. This fact suggests that all these substances originate from the indole nucleus of protein. In the animal organism indole is excreted in the urine as an ethereal sulphate (indoxyl-sulphuric acid), whereas in plants it is nearly always combined in the form of a glucoside, called indican. By the action of an enzyme (indoxylase, Beijerinck), which occurs in the leaves of indigoferous plants, the glucoside is split into dextrose and indoxyl, and the latter is oxidised, possibly by the help of a peroxydase, into indigo.

Scatole is another member of the same family, being β -methyl indole. In animals it is one of the final cleavage products of protein, and is excreted mainly in the fæces; but it also occurs in plants, and has been found by Dunstan to the extent of 1 per cent. in the wood of a Javanese tree (*Celtis reticulosa*, Miq.).

In connection with the end-products of protein katabolism another group of glucosides ought to be mentioned, which are characterised by the ease with which they split off prussic acid, when acted upon by such enzymes as emulsin. Until recently *amygdalin*, whose carbohydrate radicle still awaits identification, was the only representative of this group. The researches of Dunstan and Henry have made known quite a series of other well-characterised glucosides, of which *Dhurrin* from *Sorghum* plants contains oxymandelic acid, whilst *Phaseolunatin* (from *Phaseolus lunatus*) gives rise to acetone, in addition to dextrose and prussic acid.

Glucosides containing both nitrogen and sulphur are found widely distributed in plants. It is one of the interesting problems of bio-chemistry to trace their relationship to the cystine radicle of the protein molecule. These glucosides occur mainly in the seeds and vegetative organs of the Cruciferae, which usually also contain, in separate cells, a hydrolytic enzyme, called "myrosin," and Gadamer has lately studied very completely the two best-known glucosides of this type found in mustard seeds. *Sinigrin*, from black mustard seeds, is split by myrosin into mustard oil (allylsulphocyanide), dextrose, and

acid potassium sulphate, whilst the crystallised glucoside from *Sinapis alba*, called *Sinalbin*, gives rise to p-oxy-toluol—mustard oil, and to the acid sulphate of the base *sinapine*, besides dextrose. The base sinapine is easily hydrolysed into choline and sinapic acid, and sinapic acid has been lately synthesised by Graebe and Märtz, after having been recognised by Gadamer as a derivative of dimethyl-pyrogalllic acid.

It is questionable, however, whether all these substances are merely excretory and final products of protein cleavage. Their formation and presence may have quite a different significance. Naegely has suggested that they are weapons of defence against the attacks of bacteria. The enzymes which give rise to prussic acid, benz-aldehyde, mustard oil, etc., are only set free after injury to the cells which contain them, and the substances liberated comprise some of the most active antiseptics and bactericidal agents known.

Phyto-toxins.—We are thus led in logical sequence to consider next another class of substances, intimately related to vegetable proteins, namely the phyto-toxins. These highly poisonous substances occur preformed in plants, and are by most observers regarded as proteins; they give the protein reactions; they can be salted out of their solutions in definite fractions, are precipitated by alcohol and destroyed by proteolytic enzymes. Their protein nature has, however, been questioned, and two other views have been advanced: one of these is that the toxic principle is simply carried down mechanically with the protein, whilst according to the other the toxic principle is of the nature of an enzyme.

Against the first of these views it may be urged that the first fraction produced by salting out (the globulin), which usually carries down all the impurities, does not contain the toxin; and against the second view, we have the fact that the phyto-toxins produce their effect according to the law of definite proportions, which is quite different from the laws which regulate the catalytic action of enzymes.

The best known phyto-toxins are the following:

1. *Ricin*.—This occurs in the seeds of the castor-oil plant (*Ricinus communis*). It was first isolated by Stillmarck in Kobert's laboratory. He considered it to be an albumose on account of the way in which it could be salted out from solution, but his substance was probably a mixture. Osborne, Mendel, and

Harris have lately succeeded in isolating a very pure ricin, associated inseparably with the coagulable albumin. Its toxicity was so great that $\frac{1}{1000}$ mg. (=0.000001 gramme) per kilo. of rabbit proved fatal. The fact that its toxic action disappears after digestion with trypsin and after heat coagulation is strongly suggestive of its protein nature.

2. *Abrin*.—This toxin is contained in the seeds of *Abrus precatorius*, a tree growing in tropical countries. The seeds are known as Jequirity beans. In 1889 Sidney Martin isolated their toxic principle, and considered it as a mixture of a globulin and an albumose. It is also highly poisonous.

3. *Crotin* has been prepared in a similar way to ricin and abrin from the seeds of *Croton tiglium* (Malabar beans).

4. *Robin* is contained in the bark of *Robinia pseudo-acacia*. It has been isolated by Power, who considered it to be nucleoprotein. It shows all the protein reactions, and its aqueous solution coagulates on boiling, thereby losing its toxicity.

A true phyto-toxin of a protein nature called *Phallin* seems to be contained in the mushroom *Amanita phalloides*, different in its action and chemical behaviour from the alkaloid muscarine found in *Amanita muscaria*. The toxin causing hay-fever must also be mentioned in this connection. According to Dunbar this also is a protein (albumin) contained in the pollens of various grasses and cereals.

These phyto-toxins have been of the greatest use in the study of immunity, since their action is similar to that of animal and bacterial toxins. Like the latter they give rise to hæmolysis and agglutination, and as these phenomena can be studied outside the body in test tubes, they have been especially valuable for systematic investigations. Ehrlich showed in 1891 that it is possible to immunise animals against these toxins, and this observation furnished the starting-point for his fundamental investigations regarding immunity. Immunisation is obtained not only by subcutaneous injection, but also by feeding or by direct application to the eye. The anti-bodies formed in the blood plasma of the immunised animal are also of protein nature, and may be fractionated by $\frac{1}{4}$ — $\frac{1}{3}$ saturation with ammonium sulphate. It is interesting to note that the animal organism is only able to produce these "anti-toxins" against poisons of a protein-like character. All attempts to produce in the organism an anti-toxin against poisons of known chemical

constitution such as the alkaloids (nicotine, morphine, etc.), or against metallic poisons (arsenic, sublimate, phosphorus, etc.), have so far not been successful.

In connection with phyto-toxins, we may shortly discuss from the chemical point of view one of the many animal toxins, namely that contained in the poison fangs of certain snakes. According to Weir Mitchell and Reichert these toxins are also of a protein nature, being mixtures of a globulin and an albumose. They have recently aroused great interest through their relation to bacterial toxins and the problems of immunity. Their action may also be investigated outside the body by their hæmolytic properties, *i.e.* their effect of producing solution of red blood corpuscles. The venom which has been studied most thoroughly in this direction is that of the Indian cobra, and the results promise to be of great importance in widening our knowledge of a class of substances about the functions of which, in the body, we have been up to the present absolutely in the dark. The substances referred to are represented by cholesterin, lecithin, and allied substances, for which, following the proposal of Overton, the term "Lipoids" is now generally accepted.

Toxolecithides.—The experiments of Flexner and Noguchi were the starting-point of these observations. They found that the hæmolytic action of cobra venom on red blood corpuscles can be prevented by complete removal of the adhering serum by washing with physiological salt solution. On adding serum or blood to the washed blood corpuscles in presence of cobra poison, hæmolysis takes place. Kyes was able to explain this striking fact. Evidently serum contains an activating substance, and it was shown that this substance is heat resisting. Kyes further showed that the activator is not only soluble in alcohol but also in ether, and he finally identified it as lecithin. He was able to prove by purely chemical methods that cobra venom combines with lecithin to form the new hæmolytic compound, which he actually isolated. Most suggestive in this connection is the behaviour of cholesterin. This substance has the property of preventing or counteracting the activating action of lecithin on cobra venom. This antagonism between lecithin and cholesterin points to a chemico-physical relationship between the two substances. In order to exercise this function the hydroxyl group as well as the double linkage of carbon atoms in derivatives of cholesterin

must remain intact (Hausmann, Abderhalden and Le Count). The antagonistic action of lecithin and cholesterin has been strikingly demonstrated by Pascucci in his experiments with artificial blood corpuscles. (Glass cells containing hæmoglobin solution and covered with a membrane impregnated with lecithin, cholesterin, etc., were suspended in the toxin solution, and the diffusion of the pigment taken as an index of hæmolysis.) The poison of scorpions and bees behaves in the same way towards lecithin (Morgenroth and Carpi).

It is still too early to form a definite opinion about the chemical nature of these *Toxolecithides*, as they have been called. It has been suggested that the action of toxins is partly a lipolytic one. In this connection it is interesting to note that oleic acid and oleates (soaps) play a rôle in hæmolysis and bacteriolysis (Liebermann, Noguchi). The analytical figures furnished by cobra lecithide, which agree with the formula of lecithin minus a fatty acid radicle, would seem to be in accord with a lipolytic action of cobra venom.

Lipoids.—The chemical investigations on the isolation, properties and constitution of the lipoids have received a fresh stimulus through the above observations, after having attracted relatively little attention during the latter part of the last century. This neglect was no doubt partly due to the harmful influence of the "Protagon" idea, introduced by Liebreich in 1865. According to this theory all the constituents of nervous tissue (known at this time as phosphorised fats, lecithin, cerebrin, etc.) do not exist preformed, but are derived from the decomposition of the one and only mother substance, which was therefore called "Protagon." This product was supposed to possess a definite chemical composition, but has now been finally proved to be a mechanical mixture (Gies, Rosenheim and Tebb). The general acceptance of the "Protagon" idea until quite recently, notwithstanding the strenuous opposition of Thudichum, has retarded considerably the progress of our knowledge not only of the substances constituting the mixture "Protagon," but of the lipoids generally. Thudichum's observations are being confirmed by modern workers in this field, and the classification proposed by him is now generally accepted. It is not only applicable to the lipoids of the brain, but also to those contained in cells generally.¹ A short discussion of the substances be-

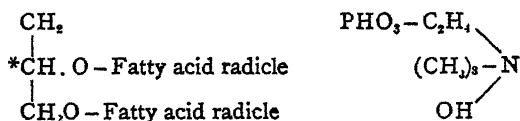
¹ See I. Bang, "Biochemie der Zell-Lipoide," *Ergebn. d. Phys.* 1907.

longing to the three first groups of Thudichum's classification seems therefore justified in this connection. These groups comprise :

1. The Phosphatides (containing P and N, such as lecithin, kephalin, etc.).
2. The Cerebrosides (P free, but containing N, such as phrenosin, kersin).
3. The P and N free substances (cholesterin, inosite, etc.).

1. The *Phosphatides* are divided into sub-groups according to their N : P ratio. The best known substances belong to the two groups of (a) mono-amino-phosphatides (N : P = 1 : 1) and (b) di-amino-phosphatides (N : P = 2 : 1).

Lecithin (or rather the lecithins) belongs to the first group of mono-amino-phosphatides, and although we are able to form what seems to be a fairly accurate picture of its constitution, it is doubtful if any worker has ever isolated a pure lecithin. It represents a waxy, hygroscopic, white or yellowish mass which easily oxidises on exposure to air. On hydrolysis by means of baryta, it gives rise to glycerophosphoric acid, choline, and fatty acids, and from these facts the following formula has been derived :



The fatty acid radicle may be variable. Stearic, palmitic, oleic and linoleic acids have been obtained, and our knowledge about the distribution of these acids in different lecithins is very vague.

Ulpiani has recently demonstrated the optical activity (dextro-rotation) of lecithin from eggs. As the natural glycerophosphoric acid, isolated from lecithin, was also found to be optically active by Willstätter, it must possess an asymmetric C-atom as indicated by the asterisk in the formula just given. Meyer was able to convert the optically active lecithin into the racemic inactive modification, and succeeded in obtaining from it the lævo-rotatory form by the action of lipase.

From a biological point of view it is interesting that phosphatides combine easily with many organic substances such as carbohydrates, proteins, glucosides, alkaloids, and some of these combinations occur preformed in animal and plant organs. The

exact nature of these substances is still unknown, the general view being that they represent merely additive compounds due to the adsorbing power of the colloidal phosphatides. The lecitho-proteins contained in the yolks of eggs have received most attention. Hoppe-Seyler isolated from egg-yolk *Vitellin*, a protein showing a globulin nature, whilst R. H. A. Plimmer has recently obtained another lecitho-protein from it, which in distinction from vitellin is soluble in water. *Jecorin*, isolated by Drechsel from liver, represents a compound of dextrose with an unknown phosphatide, always containing sulphur. The phosphatides of plants, which have been up till now considered as lecithins, contain in some cases as much as 16 per cent. of a reducing sugar (galactose), as has recently been shown by Winterstein.

As a representative of the second class of phosphatides, the di-amino-phosphatides, *Sphingomyelin* may be mentioned, which was isolated by Thudichum from brain and which represents the main phosphorised constituent of "Protagon." A similar substance has recently been isolated by Bang and Forssmann from the lipoids of red blood corpuscles, by Stern and Thierfelder from egg-yolk, and by Erlandsen from heart muscle.

2. The *Cerebrosides* are nitrogenous, but phosphorus free, white crystalline substances which are nearly always found together with the phosphatides. They are glucosides and furnish on hydrolysis galactose together with fatty acids and bases. The nature and constitution of the two latter cleavage products have not yet been cleared up. *Phrenosin*,¹ and probably *Kerasin*,² are representative substances of this type, and they constitute roughly 70 per cent. of the "Protagon" mixture.

3. The most important member of the third group of Thudichum's classification is *Cholesterin* (cholesterol). It forms a constant and evidently essential constituent of protoplasm, and is found regularly in animal as well as in vegetable cells. In distinction from the phosphatides and cerebrosides it is easily obtained in a pure crystallised state. The identity of cholesterin from different organs and animals seems to be established. A substance has, however, been isolated, so far

¹ This name, introduced by Thudichum, is preferable to the better known term "cerebrin," as the many different substances described by this name in the literature are mixtures of partly hydrolysed cerebrosides (being prepared by boiling nervous tissue with baryta). Gamgee's "pseudo-cerebrin" and Thierfelder's "cerebron" are identical with phrenosin.

only from the fat of sheep's wool (lanolin), which although isomeric with cholesterin has different properties. It is dextro-rotatory, whilst cholesterin is lævo-rotatory, and has been called *Isocholesterin* by Schulze.

The cholesterins of plants, usually called *Phyto-cholesterins* or *Phytosterins*, are less uniform, differing from each other and from cholesterin in melting point, optical activity, and colour reactions. Until recently they were considered to be definite chemical compounds; but Windaus and Hauth have shown that whilst this is true for sitosterin (from wheat), others, like phytosterin from Malabar beans or rape-seed oil, are mixtures of two substances of different optical activity and melting point.

With regard to the chemical constitution of cholesterin, all that was known a few years ago was that it had the formula $C_{27}H_{44}O$ (or $C_{27}H_{46}O$) and that it represented an unsaturated monatomic alcohol. These conclusions are based on elementary analysis, and on the formation of bromine addition compounds and ethers. These ethers, especially the oleic ether, occur preformed in the organism (in blood serum [Hürthle] but not in brain [Bunz, Tebb]), and are of special interest as having led O. Lehmann to the discovery of "*Fluid Crystals*." This phenomenon, which has not yet sufficiently attracted the attention of biologists, explains the peculiar "Myelin" forms, which, according to Lehmann, are deformed liquid crystals of oleates of ammonia and other bases.¹

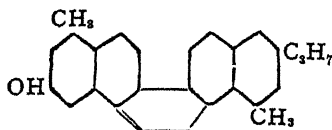
Our knowledge of the constitution of cholesterin has made great progress during the last few years mainly owing to the researches of Mauthner, Windaus, Abderhalden and Diels and others. It is now certain that cholesterin has nothing in common with fats, carbohydrates, or proteins, but that it belongs to that peculiar class of substances which are called "terpenes." The terpenes have so far only been known to occur in plants, and the proof of the terpene nature of cholesterin establishes a further link between animal and plant biology.

The *Terpenes* represent the final products of reduction (in the chemical sense) of plant metabolism and occur chiefly in plant secretions. They possess the general empirical composition of $C_{10}H_{16}$ (or $C_{10}H_{18}O$), and are converted by oxidising agents into derivatives of the aromatic hydrocarbon cymene (p-methyl-propyl

¹ See O. Lehmann, *Flüssige Krystalle*, Leipzig, 1904; also R. Schenck, *KrySTALLIN. Flüssigkeiten*, 1905, and O. Vorländer, *Krystallflüssige Substanzen*, 1908.

benzene). They are therefore considered as reduced cymene derivatives, and di-, tetra- and hexa-hydrocymenes are known. By the linking up of two or more reduced benzene rings the more complicated terpenes are built up (sesqui-terpenes, poly-terpenes). Some of the poly-terpenes show the same colour reaction as cholesterin.

Recent researches have made it very probable that in cholesterin five reduced rings are linked together, and the following formula has been tentatively put forward as explaining satisfactorily our present knowledge of the substance :



Functions of Lipoids.—With regard to the biological position of the lipoids we can infer from their general occurrence that they are of fundamental importance for the living organism, and there seems to be a good deal of truth in the opinion of Bang, that the importance of proteins as carriers of life ("Träger des Lebens") has been over-estimated, whilst that of the lipoids has been neglected. The phosphatides, from their composition, may act as intermediary agents between different groups of compounds. We easily recognise a relationship to fats, from which perhaps they are formed, the fats contributing two of the constituents of lecithin, the fatty acid and glycerin. During the process of digestion lecithins also behave like fats. They are hydrolysed by the action of lipase, the fat-splitting enzyme of the pancreas (Meyer). The question has, however, not yet been decided if the hydrolysis in the intestinal canal is a complete one, or if unchanged lecithin is absorbed as such. It is, however, probable that complete cleavage does take place. It is pretty certain that all ferments manifest a reversible action; this has certainly been proved in the case of lipase; moreover, lipase appears to be widely distributed in the tissues, and so it may be assumed that a synthesis of lecithins from their cleavage products takes place in the body. The general occurrence of another cleavage product of lecithin, namely choline, in nearly all organs where cell activity is marked, may be related to this question (Rosenheim). From another point of view the phosphatides may form a link with the cell nucleins, which possibly obtain their necessary supply of phosphorus from this source.

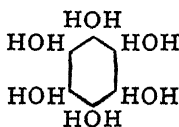
But the facts already mentioned in connection with the activating power exercised by lecithin on cobra venom and other toxins furnish us with the best clue as to its functions. The assimilation of a poisonous protein which leads to the destruction of the cell with which it becomes united, is believed to be on all fours with the assimilation of a nutritive protein which leads to the growth of a cell, or to its repair after it has undergone the waste which accompanies activity. If this doctrine is accepted, it is quite possible that lecithin may be not only an activator for the assimilation of a toxin, or an amboceptor which anchors the toxin on to the cell it attacks, but in a more general sense it may play a *rôle* in the same sort of way in relation to the safe anchorage of nitrogenous substances of food value, which are important for cell life. The latest researches on ferment action have clearly shown that ferments are not present in cells in the active state but as so-called proferments. To produce their effect another—chemically so far unknown—substance is necessary; and that this substance may be lecithin (or a similar lipid) is a view that is gaining ground. The beneficial effect, especially on nitrogenous metabolism, of small therapeutic doses of lecithin in cases of malnutrition, which has repeatedly been stated to occur, may perhaps be explained in this way. In the light also of the work previously alluded to, on the antagonistic action of cholesterin and lecithin, the normal function of the former substance may be a protective one; it is one more of the numerous chemical defences the body possesses against the attacks of various toxins, produced either by bacterial action or by normal metabolic processes in the body (auto-toxins). Some results obtained in Salkowski's laboratory by Pribram furnish indications in this direction. It was found that by feeding animals with cholesterin the percentage of this substance in the blood rose, and that the serum possessed a resistance against the hæmolytic action of saponin which was eight times greater than in the normal animal.

If it is at all permissible to draw any general conclusions from this newly acquired knowledge, which is still in a state of evolution, it would seem that a nicely balanced action of the two substances, lecithin and cholesterin, would be of the greatest importance for the maintenance of the normal healthy state of the organism.

Inosite, although not strictly belonging to the lipid substances, is included in the third group of Thudichum's classi-

fication, and deserves a short discussion as a substance common to both animals and plants. Since its discovery in muscle by Scherer in 1850 it has also been known as muscle sugar, possessing the same molecular formula as the simple sugars $C_6H_{12}O_6$.

It has, however, except its sweet taste and empirical formula, nothing in common with sugars ; but Maquenne showed in 1887 that the constitution of a reduced hexa-oxy-benzene must be ascribed to it :

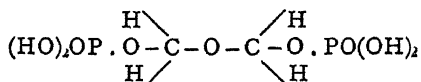


It represents one of the few known cases of optical activity in the absence of an asymmetric carbon atom, as foreseen by Van't Hoff and Le Bel's theory. Nine forms are possible, of which two optically active and one inactive modification are known. Inosite has been found since its discovery in nearly every animal organ (brain, heart, spleen, kidney, etc.). In plants it also occurs as a general constituent of roots and leaves, especially growing leaves.

Although we are still without any definite knowledge as to its functions in the animal body, we may assume that it represents a transition stage from the carbohydrates to the benzene derivatives. By a closing-up of the open chain of the carbohydrate molecule its formation is theoretically possible from the latter. On the other hand, the opening of the ring would give rise to an open chain, and it has indeed been found that lactic acid is formed from inosite by bacterial action—a fact recently confirmed by Harden and Young.

Organic Compounds of Phosphoric Acid.—A much clearer idea, if not about its functions, so at least about the formation of inosite in the plant, has been obtained through recent researches. Winterstein was the first to notice that mustard seeds contained the magnesium salt of an organic compound of phosphoric acid, which on boiling with acids gives rise to inosite. Independently of Winterstein, the same substance was also obtained by Posternack, who was able to isolate it from leguminous seeds, cereals, many oily seeds, and rhizomes. According to him, the globoid of the aleurone grains represents the source of this substance. Its chemical investigation showed an unexpected

result—namely, that on hydrolysis phosphoric acid and inosite are produced, these being the only two cleavage products—and Posternack represented its constitution by the formula of an anhydro-oxy-methylen-phosphoric acid :



according to which it contains the molecules of formaldehyde and phosphoric acid. This formula explains the inosite formation, for three molecules of the acid would give rise to inosite and phosphoric acid. The suggestion has, however, also been made lately to assume a preformed inosite molecule.

The investigation of the quantitative occurrence of this substance, for which the short name "*Phytin*" has been introduced, showed that 80—90 per cent. of the total phosphorus of seeds is present in this form, the rest being mostly in lecithin. The explanation which Posternack gives for the formation of this substance in the plant is suggestive. As already demonstrated by Schimper, phosphoric acid is transformed in the leaves into organic phosphates, and this transformation depends on the activity of chlorophyll under the influence of light. We know, on the other hand, that the products of this "photosynthesis" are transported as soon as they are formed to the parenchymatous and embryonic cells of the plant and deposited in the organs which serve as the storage places for reserve material. Posternack concludes therefore that the formation of this substance takes place during the actual process of carbonic acid reduction by chlorophyll, and that formaldehyde is the initial substance formed which combines at once with phosphoric acid to form the new compound. During germination of the seeds large quantities of inosite are formed from their phytin store, and a ferment (phytase), which is responsible for this transformation, has been isolated from the husks of rice, which contain about 8 per cent. of phytin (Suzuki and Yoshimura). According to McCollum and Hart, the blood and liver of animals also contain a phytase, whilst muscle and kidney extracts are without action on phytin. Gilbert and Posternack maintain that the animal organism absorbs and retains a large amount of this organic phosphorus compound. In connection with phytin, which represents a vegetable organic phosphoric acid, an animal representative of this type may be mentioned about which

important information has been gained within the last year or two: *inosic acid*. This substance was discovered by Liebig in muscle and subsequently in meat extract. In overlooking its phosphorus content, Liebig made one of his few mistakes. Inosic acid is regarded as the simplest representative of the nucleic acids, and it is the only one which can at present be obtained pure (as a crystalline magnesium salt). Last year Neuberg and Brahn, and independently Bauer, succeeded in clearing up the constitution of this interesting compound. On hydrolysis it furnishes a carbohydrate, a purine base and phosphoric acid. The purine base was identified as hypoxanthine and the carbohydrate as a pentose (either l-xylose or i-arabinose). On partial hydrolysis, Neuberg and Brahn obtained a pentose phosphoric acid. Similar ethereal compounds between carbohydrates and phosphoric acid seem to be formed during fermentation of sugars by zymase in the presence of inorganic phosphates (Young, Iwanoff).

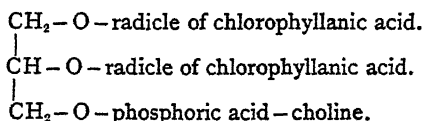
Chlorophyll.—As the organic phosphoric acids, and especially phytin, have been brought into close relationship with chlorophyll, an account of the progress made in the chemical knowledge of this important respiratory pigment may be justified.

Since the name "chlorophyll" was given to the green pigment of leaves by Pelletier and Caventou in 1817, no substantial progress had been made in its chemistry until Hoppe-Seyler, the pioneer of modern biological chemistry, obtained crystalline derivatives from it. Most of our present knowledge of chlorophyll is based upon the methods which he introduced. The work of Schunck and of Marchlewski, in Manchester, is also of fundamental importance. The researches of Willstätter, which were only published last year, are based on ingenious new methods. It is perhaps too early to estimate the value of his work, though it may be the starting-point for new discoveries in connection with the chemistry of chlorophyll.

The pure native chlorophyll pigment has so far probably never been isolated by anybody. It is a substance of such a labile character, and so easily affected by light and air, that all attempts to separate it have been in vain, and the statements about pure crystallised chlorophyll in the vast literature of the subject are still awaiting confirmation.

The whole of our knowledge about chlorophyll is based on the study of its cleavage products, produced by the action of acids

or alkalies, and dates back to Hoppe-Seyler. He obtained the first crystallised chlorophyll derivative, which he called *Chlorophyllan*, by extracting grass with boiling alcohol. This substance, according to him, is a lecithin derivative, in which the fatty acid radicles are replaced by chromophoric groups of acid character. These groups he called chlorophyllanic acids. The simplest structural formula of it would be :

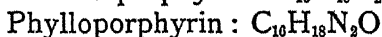
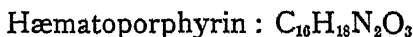


It is, however, very doubtful if the constitution of the substance is so relatively simple. It always contains mineral substances, especially magnesium, which seems to be an integral constituent.¹ Schunck and Marchlewski considered it as mainly composed of the two cleavage products, phyllocyanin and phylloxanthin, whilst on the other hand Stoklasa could not obtain crystallised chlorophyllan by Hoppe-Seyler's method. He concludes, however, that the formation of chlorophyll is intimately connected with the presence of phosphorus : "without phosphorus no lecithin and also no chlorophyll." Willstätter could only find relatively small amounts of phosphorus, and was unable to obtain glycerin as a cleavage product. He isolated, however, a nitrogen free alcohol, $\text{C}_{20}\text{H}_{40}\text{O}$, and concludes that chlorophyll is an ether, probably of this alcohol, with acids which contain magnesium in a very resistant organic com-

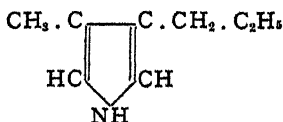
¹ It was originally held that chlorophyll, like hæmoglobin, was an iron compound. This view was, however, completely disproved by later and more careful investigation. The presence of iron seems always to be connected with oxidation processes in the animal organism, whilst in the plant, especially in the assimilation of carbonic acid by chlorophyll, we have to deal with a reducing process. In this connection the presence of magnesium in chlorophyll is highly suggestive, and another striking example of the necessity of magnesium for pigment formation is furnished by the observation that the *bacillus prodigiosus* does not produce its characteristic red pigment in the absence of magnesium salts. We always associate a reducing action with this metal, and this function is perhaps the explanation of its presence in the vital organs of the plant. The failure of chemists to imitate nature in the reduction of carbonic acid to formaldehyde has always been an argument against Baeyer's theory of formaldehyde formation as the first intermediary assimilation product of plants. It is instructive to note that Fenton, by using magnesium as the reducing agent, has succeeded where so many have failed.

bination. This is not the place to enter into a detailed description of the many well-crystallised derivatives of chlorophyll, the knowledge and spectroscopic examination of which we owe chiefly to Schunck and Marchlewski.¹

Relationship between Chlorophyll and Hæmoglobin.—From our point of view the most interesting derivative is the end product phylloporphyrin, which is obtained from all chlorophyll derivatives by the energetic action of alcoholic potash at 190°. It crystallises well and forms blood-red solutions, showing seven well-defined absorption bands. Schunck and Marchlewski had previously pointed out the great similarity of the spectra of phylloporphyrin and hæmatoporphyrin, the iron-free constituent of blood pigment, and Nencki drew attention to the slight difference in the molecular formulæ of the two substances:



which suggests that hæmatoporphyrin is a di-oxy-phylloporphyrin. If this view is correct, it should be possible to transform the one into the other, either by reduction of hæmatoporphyrin or by oxidation of phylloporphyrin. It was eventually found that both porphyrins furnished the same derivative on complete reduction. This is hæmopyrrol (methyl-propyl-pyrrol)—



and was obtained in 1901 by Nencki and Zaleski, from hæmatoporphyrin, and a little later by Nencki and Marchlewski from phylloporphyrin.

The relationship of the animal and vegetable respiratory pigment was thus for the first time clearly established. The presence of the pyrrol ring in these pigments also suggests a relationship to the proteins, in which E. Fischer has demonstrated the presence of the pyrrolidine ring.

A further result of the work of the Russian investigators was that the final reduction product of both blood and plant pigment, hæmopyrrol, was easily transformed on exposure to air into

¹ See Marchlewski, *Die Chemie des Chlorophylls*.

urobilin, the iron-free pigment found in bile and urine after hæmorrhage, giving all the characteristic reactions and the spectrum of the natural substance. On subcutaneous injection of hæmopyrrol, urobilin could be easily demonstrated in the urine.

In the beginning of last year Marchlewski brought forward a further most interesting proof of the near relationship between the two pigments. A few years ago Zaleski succeeded in reconstituting hæmin by introducing iron into hæmatoporphyrin (or rather into the more active mesoporphyrin). The artificial hæmin possessed the same crystalline form (Teichmann's crystals), and practically the same spectrum as ordinary hæmin. At about the same time Laidlaw, in Cambridge, published similar results, and he succeeded in preparing artificially the brilliant red pigment *Turacin*, contained in the wings of certain birds, by combining copper with hæmatoporphyrin. Working on the same basis, Marchlewski was able to introduce iron into phylloporphyrin, and thus obtained a *Phyllohæmin*, which gave the same spectrum as hæmin. This is the first instance of the transformation of a chlorophyll derivative into one of hæmoglobin.

This last and perhaps most striking proof of the near relationship of vegetable and animal life brings to an appropriate conclusion my endeavour to present some of the problems of bio-chemistry, in which both forms of organic nature have a share. A comparative study of the chemical biology of plants and animals deserves more attention than it has hitherto received. It not only brings home to us the resemblances, but at the same time helps us to arrive at a clearer understanding of the fundamental biological differences between the members of the two forms of life.

In view of the absolute dependence of animal on plant life, considerations of this nature may not only prove to be of scientific interest, but also of economic importance.

THE PRESENT POSITION OF THE HYDRATE THEORY OF SOLUTIONS

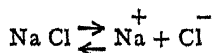
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PART I

It is characteristic of the wide difference of opinion that has existed in reference to the nature of solutions in general, and of aqueous solutions in particular, that the editor of Watt's *Dictionary of Chemistry* found it advisable in 1894 to include articles on "Solutions" by two different authors. The theory described by Arrhenius in the former of the two articles was essentially one of dissociation, it being supposed that the main effect of adding salt to water was the resolution of the salt into the ions or charged radicles which separate at the electrodes when the salt is decomposed by electrolysis. Pickering, developing in the second article a view of the nature of solutions previously elaborated by Mendeleef, urged that the main effect of bringing together the solvent and solute was to initiate a process of association whereby the salt and water combined together to form a series of hydrates, the complexity of which increased with the proportion of water added to the salt.

That the action of the solvent was incomplete and reversible appears to have been recognised, at least to some extent, in each case. Arrhenius' theory, which may be represented symbolically by the equation,



included as one of its essential features the idea of a "co-efficient of ionisation," which showed the extent to which the salt had become ionised or dissociated in solution. As the conductivity of the electrolyte was attributed wholly to the ionised portion of the salt, and ionisation was thought to be complete at infinite dilution, it was easy to deduce a value for this co-efficient by comparing the molecular conductivity of the

solution with that of the salt at very great dilutions. On the other hand, it had been found by van t'Hoff and others that, in contrast with non-electrolytes, salt solutions exhibited abnormally high osmotic pressures, just as would be the case if the constituent ions were capable of acting separately in contributing to the osmotic pressure of the solution: by following up this clue it was possible to deduce, either from direct measurements of osmotic pressure, or indirectly from observations of boiling-point and freezing-point, a second series of values for the co-efficient of ionisation of a salt. Although much collateral evidence was adduced in favour of the view that the salt-radicles existed independently in solution, the approximate agreement between the values for the co-efficient of ionisation deduced by the two methods was an experimental result of the utmost importance, and undoubtedly afforded one of the strongest arguments in favour of the dissociative theory.

In the case of the hydrate theory, on the other hand, the existence of a reverse action did not form an essential feature, but was rather a disturbing factor, the existence of which was somewhat grudgingly admitted. Like many other theories this conception of the nature of solutions had existed in a nebulous form for many years prior to the publication of the paper in which it first took definite shape. The idea that an aqueous solution might contain compounds of solvent and solute, similar to those which frequently separate when the solution is allowed to crystallise, was widely disseminated and had formed the subject of frequent and prolonged discussion. Mendeleef's paper was, however, remarkable in that it introduced a method by which the nature and composition of these hydrates could be determined from a study of the solution itself. He thus gave to the theory a definite quantitative character, and brought it, practically for the first time, within the region of exact experimental investigation. In measuring the densities of aqueous solutions of sulphuric acid and of alcohol, he had found that the curves showing the relationship between concentration and density were not continuous, but exhibited abrupt changes of direction at certain points. In the case of sulphuric acid, these changes occurred at the compositions 3'5, 47'6, 73'9, 84'5, and 100 per cent. H_2SO_4 , corresponding with the formulæ $\text{H}_2\text{SO}_4 \cdot 150\text{H}_2\text{O}$, $\text{H}_2\text{SO}_4 \cdot 6\text{H}_2\text{O}$, $\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$,

and H_2SO_4 . He therefore concluded that the breaks in the curve were due to the formation of the series of hydrates represented by the above formulæ: each section of the density curve showed the behaviour of a mixture of two hydrates, the composition of which was indicated by the breaks at either end, whilst the breaks themselves were the natural consequence of the disappearance of one hydrate from the solution and its replacement by another hydrate of different composition and different density.

The process of hydrate formation could, of course, be reversed by evaporation or distillation, but it was necessary in applying Mendeleef's method to assume that in any given solution the combination of solvent and solute was essentially complete and non-reversible. Abrupt changes in the properties of the solution were, in fact, only possible if the whole of the solvent entered into combination with the solute, and in such a way that no appreciable quantity of a third hydrate C was found until the first hydrate A had been completely converted into the intermediate hydrate B.

It is noteworthy that Mendeleef's paper is found along with the classical papers of Arrhenius and van t'Hoff in the first volume of the *Zeitschrift für physikalische Chemie*. The publication of this journal marked the inauguration of a new period of study of physical chemistry; and the present year, which celebrates its coming of age, affords a particularly opportune moment for a review of the progress which has been made by the rival theories during the intervening twenty-one years. During the major part of this period the followers of Arrhenius and van t'Hoff enjoyed almost a monopoly of publication, so far, at least, as the Continental journals were concerned; the hydrate theory was openly ridiculed by the German school of physical chemists, whilst papers dealing with the theory of electrolytic dissociation increased year by year both in number and in bulk. With the coming of the new century, however, the hydrate theory has come into its own, and in its maturer form has again formed the subject of many important researches, and has attained to almost universal acceptance. There are, indeed, few cases on record in which the elements of truth existing in each of two rival theories has worked more effectively in bringing about a final reconciliation of views which were long thought to be wholly incompatible.

It will be convenient to describe the gradual development of the hydrate theory in two sections, noting in the present communication the introduction into the theory of the ideas of mass action and reversibility, and in a subsequent article its application to the phenomena of electrolysis, and its intrusion into the hypothesis of electrolytic dissociation, together with the quantitative developments which have given to the theory a period of new vigour, and have made it one of the foremost subjects of investigation at the present time.

REVERSIBILITY OF HYDRATE FORMATION

Mendeleef and those who followed him most closely appear to have regarded the process of solution as essentially chemical in character. The water dissolved the salt because it combined with it, and this combination involved practically the whole of the water as well as the whole of the salt. Whether at extreme dilutions a mere trace of solute was to be regarded as in combination with the whole bulk of the solvent is not quite clear, but it is at least evident from Mendeleef's paper that a molecule of sulphuric acid was thought to be capable of uniting with the greater part of the first 150 molecules of water which were added to it, so that uncombined water could only be present to any marked extent in solutions containing less than 3.5 per cent. of acid. Similar views were advocated by Pickering (*Trans. Chem. Soc.* 1890, 57, 129), who suggested that $1800\text{H}_2\text{O}$ was a reasonable estimate of the complexity of the molecule of water itself, and extended Mendeleef's list of hydrates by the addition to it at one end of the series of the compounds $3\text{H}_2\text{SO}_4 \cdot 2\text{H}_2\text{O}$, $3\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$, $6\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$, and $36\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$, and at the other end of the compounds $\text{H}_2\text{SO}_4 \cdot 508\text{H}_2\text{O}$, $\text{H}_2\text{SO}_4 \cdot 1600\text{H}_2\text{O}$, and $\text{H}_2\text{SO}_4 \cdot 5182\text{H}_2\text{O}$! Intermediate compounds with $1\text{H}_2\text{O}$, $1.5\text{H}_2\text{O}$, $2\text{H}_2\text{O}$, $5.5\text{H}_2\text{O}$ (replacing Mendeleef's $\text{H}_2\text{SO}_4 \cdot 6\text{H}_2\text{O}$), $9\text{H}_2\text{O}$, $13\text{H}_2\text{O}$, $14\text{H}_2\text{O}$, $58\text{H}_2\text{O}$, and $130\text{H}_2\text{O}$ (replacing Mendeleef's $\text{H}_2\text{SO}_4 \cdot 150\text{H}_2\text{O}$) brought the total list of hydrates up to sixteen in this one series of solutions.

These extreme views as to the complexity of the hydrates existing in aqueous solutions never gained general recognition, but on the contrary were responsible for creating a widespread prejudice against a theory of solutions which was otherwise in many respects so obvious as scarcely to demand a formal proof. This prejudice was extended in an even more marked degree

to the method which was used to demonstrate the presence of the hydrates. In order to bring out the changes of curvature due to hydrate formation, Mendeleef had introduced the method of plotting the first differential co-efficient $\frac{ds}{dp}$ of the density s against the percentage composition p . The result, in the case of sulphuric acid, was a series of five straight lines of varying inclination, which did not intersect but "jumped" at the points corresponding with the composition of the four hydrates. But as Arrhenius pointed out (*Phil. Mag.* 1880, **28**, 33), the same differential co-efficients had been plotted by Mendeleef himself two years previously (*Ber.* 1886, **16**, 386) as a pair of intersecting curves. It was therefore natural to conclude that the existence of the breaks was to a large extent a matter of opinion.

This impression was considerably strengthened when Pickering, using fresh experimental data, was led to deny altogether the existence of the five straight lines on which Mendeleef had relied for the proof of his theory, and plotted the first differentials as a single curve, continuous from end to end. By differentiating this curve a second time he obtained, however, a second set of points which he considered might be represented by a series of eleven straight lines. This second differentiation was made the subject of very severe criticism. Owing to the existence of experimental errors it could only be carried out after "smoothing" the curve formed by the first differentials—a process that was evidently accompanied by considerable risk and a certain amount of arbitrariness. Moreover, even when the second differentials were plotted, many of the "breaks" were so insignificant that it would have been easy in many cases to join by a single curve points which were supposed, in accordance with the hydrate theory, to lie on a series of straight lines. It was therefore possible that the breaks in the curve of the second differentials, like those in Mendeleef's curve of the first differentials, were due to errors of measurement which might be eliminated by further improvements in the experimental methods. Nernst, indeed, in a contemptuous criticism of the "so-called" hydrate theory (*Theoretical Chemistry*, p. 445), went so far as to say that the readiness with which breaks were found was dependent mainly on the inaccuracy of the observations, and that the

poorer the experiments the greater was the "crop of hydrates" which could be obtained by Mendeleef's process of analysis; the second differentiation, introduced by Pickering, was considered to be merely a more efficient method of revealing experimental errors, which if carried out sufficiently often, would infallibly reveal breaks even in the smoothest experimental curves.

As an alternative to the method of differentiation, Pickering introduced in 1892 the use of a bent lath as a means of detecting small changes of direction and curvature (*Phil. Mag.* 1892, **33**, 436). When such changes exist there is no doubt that the use of a bent lath may serve to reveal them to the eye. But the method requires to be used with great caution on account of the risk which exists of recognising imaginary breaks in a perfectly continuous curve. It is a matter of common experience that the simple logarithmic curves which represent the course of a "monomolecular action" usually have to be drawn in three sections, even if curves of accurate mathematical form are used to guide the pen, and similar complications must ensue whenever an attempt is made to express a curve by equations other than those which correctly represent it. The bent lath, when used in the ordinary way, forms a parabola similar to that represented by the equation $y = a + bx + cx^2$, and affords a satisfactory method of drawing curves which differ but little from straight lines. Actual parabolas are, however, of rare occurrence in physical or physico-chemical measurements, and the attempts that are still frequently made to refer curves of all sorts to this one particular type have led so often to erroneous conclusions that it may be desirable to quote from actual experience one or two typical examples.

It was found by Desguisne (*Dissertation Strassburg*, 1895), following Kohlrausch and others, that the relationship between the electrolytic conductivity of aqueous solutions and their temperature could be expressed over the range from 2° to 34° by the parabolic formula

$$k_t = k_0 (1 + at + bt^2),$$

the temperatures t being measured from 18° C. The constant b was positive for the majority of salts, negative for acids, and in a few special cases where the temperature-conductivity curves were nearly straight it reached almost a zero value. The idea that a parabola was the normal type of curve was so widely diffused that when an inflected curve was found it was con-

sidered to be an abnormal type, and was quoted (Donnan and Bassett, *Trans. Chem. Soc.* 1902, 81, 953) as evidence for the existence in the solution of abnormal complex ions. A study of the phenomena in a large number of solvents and over a wider range of temperature led to the conclusion that the inflected conductivity-temperature curve was the normal type for composite electrolytes (Bousfield and Lowry, *Proc. Roy. Soc.* 1902, 71, 53), the curves for the acids (b negative) being shown to be inflected at lower temperatures (Kunz, *Comptes rendus*, 1902, 135, 788-90), and those for the salts (b positive) at higher temperatures (Noyes and Coolidge, *Zeit. phys. Chem.* 1903, 46, 323); the straight lines given by $b = 0$ were merely portions of curves which had become momentarily straight in the neighbourhood of a point of inflection.

Even this, however, did not exhaust the complexity of the curves, which at higher temperatures exhibited a maximum of conductivity and (at least, in some instances) a second point of inflection. All these peculiarities could be interpreted on the assumption that the two properties of the solution—ionisation and mobility—on which its conductivity depends, varied continuously with the temperature, but in opposite directions and according to different laws. In no one case could the whole curve be studied experimentally; but the formula

$$k_t = k_0 (1 + bt)^{n_0 - at}$$

(Bousfield and Lowry, *Proc. Roy. Soc.* 1904, 72, 281) was found to give expression to all the above peculiarities, and this in spite of the fact that the use of a bent lath or of parabolic formulæ would have necessitated the splitting up of the curve into five or six separate sections.

Curves of a similar type, each containing a point of inflection and a maximum, are obtained by plotting conductivity against concentration in mixtures of nitric acid and water, acetic acid and water, or acetic acid and pyridine; it is therefore not improbable that these curves also may be represented by comparatively simple equations. The curves for mixtures of amylamine or butylamine with acetic acid and for aniline with acetic acid (H. E. Patten, *Journ. Phys. Chem.* 1902, 6, 554), on the other hand, are obviously distorted owing to the formation of molecular compounds, and must be represented by equations of greater complexity.

As a further illustration of the danger which undoubtedly

exists, and which Pickering himself recognised,¹ that the careless use of a bent lath may lead to the recognition of imaginary breaks in a continuous (but non-parabolic) curve, reference may be made to the viscosity curves for mixtures of pyridine and water. These are very similar in form to the inflected conductivity-temperature curves, and might in all probability be represented by similar equations. It is, however, noteworthy that whilst Hartley, Thomas, and Appleby (*Trans. Chem. Soc.* 1908, **93**, 546) were able to represent their experimental points in a perfectly satisfactory manner by a single continuous curve, the previous observers (Dunstan, Thole, and Hunt, *Trans. Chem. Soc.* 1907, **91**, 1728) were led by the use of a bent lath to plot their points as lying on a series of no less than seven intersecting parabolas. The discontinuity in this case appears to have been due in part to small experimental errors; but even if these were eliminated, it would be necessary to resolve the curve into almost as many separate sections before it could be distorted successfully into a parabolic form.

But whilst the general acceptance of the hydrate theory of solutions was undoubtedly retarded by the extreme complexity of the hydrates postulated and by the uncertain character of the methods used in detecting them, a far more serious obstacle arose from the incompatibility of Mendeleef's theory and method with the ideas of reversibility and mass-action which were gradually permeating the whole of chemical science. The inspiration which led him to look for, and to detect, abrupt changes in his experimental curves was derived from a view of the process of hydrate formation in solution, which finds a close analogue in the actual behaviour of solid hydrates. Pareau had shown in the case of copper sulphate (*Ann. Phys. Chem.* 1877 (3), **1**, 39) that each solid hydrate possessed a definite vapour-pressure increasing with the temperature in the same way as in the case of water itself. At 50° the values were:

CuSO ₄	0
CuSO ₄ .H ₂ O	4.4 mm.
CuSO ₄ .3H ₂ O	30 mm.
CuSO ₄ .5H ₂ O	47 mm.
Water	91 mm.

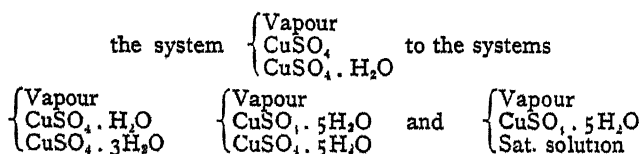
¹ "The application of the graphic method requires a great amount of care and a close attention to experimental and other conditions, and it is to be feared that the hurried use of it by those who have not taken the trouble to master the necessary details, or to acquire the requisite amount of skill, may bring it into undeserved disrepute" (*Phil. Mag.* 1892, **33**, 451).

Any attempt to increase the vapour-pressure resulted in the formation of a hydrate with a larger proportion of water, whilst the removal of water vapour, instead of producing a reduction in the pressure, merely caused a part of the hydrate to dissociate. If water vapour was added to the anhydrous salt, the vapour-pressure rose by a series of abrupt steps from zero to

4.4 mm. (CuSO_4 partially converted into $\text{CuSO}_4 \cdot \text{H}_2\text{O}$)
 30 mm. ($\text{CuSO}_4 \cdot \text{H}_2\text{O}$ " " " $\text{CuSO}_4 \cdot 3\text{H}_2\text{O}$), and
 47 mm. ($\text{CuSO}_4 \cdot 3\text{H}_2\text{O}$ " " " $\text{CuSO}_4 \cdot 5\text{H}_2\text{O}$),

intermediate pressures being possible only as a transient condition prior to the establishment of equilibrium. Further addition of water would produce a fourth "step" in the vapour-pressure, owing to the partial conversion of the pentahydrate into saturated solution, but the pressure at this stage was not measured.

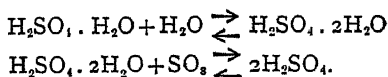
A similar state of affairs was postulated by Mendeleef in reference to hydrate formation in the liquid state, and his expectation that abrupt changes of curvature and direction would be found in the curves representing the physical properties of a series of solutions was a logical result of this view. It is indeed by no means obvious at first sight that there need be any marked difference between the conditions which determine the formation and decomposition of solid and of liquid hydrates. The hydrates of sulphuric acid are at least as stable as those of copper sulphate, as is shown by the fact that concentrated sulphuric acid is capable of destroying the blue colour of the pentahydrate. It was therefore not unreasonable to suppose that when water was added to the anhydrous acid a series of liquid hydrates would be formed one after the other in just the same manner as in the case of solid copper sulphate, the number of co-existing hydrates being limited to two. Looked at from the modern standpoint, the difference between the two cases is seen to depend mainly on the fact that liquid hydrates are, as a rule, miscible in all proportions, whilst each solid hydrate forms, as a rule, a separate "phase." If the hydrates of copper sulphate were isomorphous with one another so as to form only a single solid phase, the "stepped" vapour-pressure curve which marks the transition from



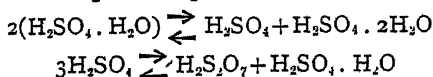
would be reduced to a series of inclines or intersecting straight lines, and this is the utmost that can be expected in the case of liquid hydrates which do not form separate liquid layers.

In addition to the question of isomorphism there is, however, an even more important point of contrast between solid and liquid hydrates in reference to the effects produced by dissociation in the two cases. Suppose that a hydrate $\text{CuSO}_4 \cdot 4\text{H}_2\text{O}$, instead of producing a vapour-pressure intermediate between those of $\text{CuSO}_4 \cdot 3\text{H}_2\text{O}$ and $\text{CuSO}_4 \cdot 5\text{H}_2\text{O}$, produced a greater vapour-pressure than either. The addition of water vapour to the trihydrate would evidently convert it into the pentahydrate rather than into the tetrahydrate, and even if the tetrahydrate could be prepared it could only exist in a metastable condition, and would be liable at any time to decompose or dissociate into a mixture of the tetrahydrate and pentahydrate. As only one vapour-pressure is possible in the system, the dissociation would proceed until the whole of the tetrahydrate had been decomposed.

In the case of a mixture of liquid hydrates, on the other hand, considerations of mass-action come into play. The rate at which the less stable hydrates decompose decreases as the concentration diminishes, whilst the more stable hydrates decompose with increasing rapidity as they become more and more predominant in the solution. Ultimately, therefore, a point is reached at which even the least stable hydrates are capable of existing, although at greatly reduced concentrations, as permanent constituents of the solution. This implies that a hydrate such as $\text{H}_2\text{SO}_4 \cdot 2\text{H}_2\text{O}$, which as a solid could only co-exist with one of the adjacent hydrates, H_2SO_4 or $\text{H}_2\text{SO}_4 \cdot 2\text{H}_2\text{O}$, may as a liquid exist in contact with a wide range of hydrates, and may even be present in small quantities in solutions containing free water or free sulphur trioxide owing to the reverse action in the processes.



It follows also that whereas in the solid state dissociations such as those represented by the equations

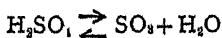


must proceed either to completion or not at all, in the liquid state

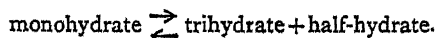
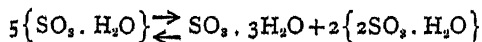
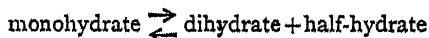
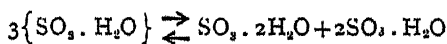
dissociation need not be complete even in the case of the least stable compounds, and on the other hand cannot be disregarded even in hydrates of exceptional stability.

The theory of solutions which results from considerations of this sort differs widely from the hydrate theory as originally formulated by Mendeleef, and the frank recognition that the formation of hydrates in solution is an incomplete and reversible process has been one of the most important factors in winning for it the general acceptance which it enjoys at the present time.

The form which the theory now takes may be illustrated by referring again to the hydrates of sulphuric acid. That sulphuric trioxide and water when mixed in molecular proportions unite to form the compound $\text{SO}_3 \cdot \text{H}_2\text{O}$ is evident from the fact that at temperatures below $10^{\circ}35'$ a crystalline product is obtained which answers to all the ordinary tests for a chemical compound. When the crystals are melted it is probable that dissociation at once sets in, though only to a very slight extent. As the temperature rises dissociation increases, so that at comparatively low temperatures fumes of sulphuric trioxide are already liberated from the liquid; at 338° the liquid boils, but the vapour consists, not of the hydrate H_2SO_4 , but of the dissociation product SO_3 . The change represented by the equation



is then of dominant importance, but its existence must be recognised over the whole range between the freezing-point and the boiling-point of the acid.¹ At low temperatures other types of dissociation must be recognised, giving rise to various hydrates containing larger and smaller proportions of water, e.g.



These must all be regarded as possible constituents of the melt, although it is impossible at the present time to say which are

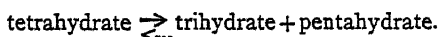
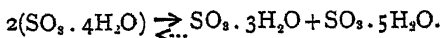
¹ Compare the case of nitrogen peroxide, where the colourless ice melts to a pale straw-coloured liquid owing to slight dissociation of N_2O_4 into NO_2 .

the main products of dissociation or to what extent they are produced under any given conditions.

The dihydrate $\text{SO}_3 \cdot 2\text{H}_2\text{O}$ is in many respects similar to the monohydrate. It is a beautifully crystalline compound melting at 8.53°C ., but appears to be less stable than the monohydrate. It boils at 220° , but the vapour consists of water and not of sulphur trioxide. When melted, it must be supposed to dissociate in much the same way as the monohydrate, but to a greater extent, so that the products of dissociation, instead of forming only a minute percentage of the mixture, may be present in very considerable quantities. The nature of the dissociation products will be similar to those formed from the monohydrate, but sulphur trioxide and the half-hydrate will only be present in small proportions, whilst water and the more aqueous hydrates will be important constituents of the mixture.

The hydrates $\text{SO}_3 \cdot 3\text{H}_2\text{O}$ and $\text{SO}_3 \cdot 5\text{H}_2\text{O}$, melting at -39° and -25° respectively, are even less stable than the dihydrate, and it is probable that by the time they have reached atmospheric temperatures they are already dissociated to such a large extent that the products of dissociation are already major constituents of the solution, present in quantities comparable with those of the undecomposed hydrate.

No other hydrates have yet been separated from mixtures of sulphur trioxide and water, though many others may exist which are either too soluble or present in too small proportions to crystallise out from the melt. In this category may be placed the intermediate hydrate $\text{SO}_3 \cdot 4\text{H}_2\text{O}$ and a long series of hydrates containing larger proportions of water. In the case of the simpler hydrates it is probable that marked differences may exist between the stabilities of adjacent members of the series, so that if the tetrahydrate were a compound of small stability a solution of the composition $\text{SO}_3 \cdot 4\text{H}_2\text{O}$ might consist mainly of the hydrates $\text{SO}_3 \cdot 3\text{H}_2\text{O}$ and $\text{SO}_3 \cdot 5\text{H}_2\text{O}$ in approximately molecular proportions.



But it is doubtful whether amongst the more complex hydrates such marked differences exist, and it is very unlikely that in a solution containing 3.5 per cent. H_2SO_4 the main constituent would be (as Mendeleef supposed) the hydrate $\text{SO}_3 \cdot 15\text{H}_2\text{O}$,

to the practical exclusion of the hydrates $\text{SO}_3 \cdot 15\text{H}_2\text{O}$ and $\text{SO}_3 \cdot 152\text{H}_2\text{O}$. The tendency at the present time is rather to regard the sulphur trioxide or sulphuric acid molecules (or ions) in a dilute solution as nuclei round which water molecules are condensed. The size of the "watery atmosphere" (F. Kohlrausch, *Proc. Roy. Soc.* 1903, **71**, 338) may vary with the concentration, pressure, and temperature of the solution (Bousfield, *Phil. Trans.* 1906, **206**, 101), and similar differences may exist between individual molecules in the same solution; although therefore at any one moment a molecule of acid might be associated with a definite number of water molecules, the number would vary with each collision, and the conception of a series of hydrates present in definite proportions under definite conditions could have only a statistical meaning.

The introduction into the hydrate theory of the idea of reversibility had the result of destroying the theoretical justification for Mendeleef's breaks—a justification that was of exceptional importance in view of the doubt attaching to their actual existence. The abrupt changes of direction which he expected to find in his experimental curves are of frequent occurrence when saturated solutions are studied, although this involves working at the temperature of crystallisation instead of a constant temperature of 20° or 50° . Thus when a substance such as magnesium sulphate crystallises from its solution with different proportions of water the break in the solubility-temperature curve which marks the appearance of a new hydrate is accompanied by a corresponding break in the density-temperature curve, although the isothermal density-concentration curves may be perfectly regular. But in the case of solutions which are not saturated similar breaks can only occur when compounds are formed which do not dissociate when fused, a condition that is altogether exceptional in the case of aqueous solutions, and would be even rarer in the case of less active solvents.

But although dissociation is fatal to the appearance of abrupt breaks in the properties of a series of solutions, it must be recognised that very rapid alterations of direction and curvature may occur when hydrates of great stability are formed. This appears to have been the view entertained by Pickering, who admitted that the hydrates of sulphuric acid might dissociate in solution of their own accord without resorting to the extreme

process of distilling off the water. But, while he thus paid formal homage to the idea of mass-action, the effects which it produced were treated as of very slight significance. The points obtained by differentiating the experimental numbers were represented by sharply intersecting curves, and their essential continuity was treated as a matter of remote and theoretical, rather than of immediate and practical, importance. It is, moreover, noteworthy that the breaks which he recognised were not limited to a small number, such as might be produced by the formation of a few hydrates of exceptional stability, but were recorded at sixteen different concentrations between 0.104 and 99.493 per cent. H_2SO_4 . The admission that dissociation might occur was thus accompanied, not by a restriction of Mendeleef's method, but by its application to cases even more extreme than those which Mendeleef had himself considered. It will therefore be of interest to consider, both from the theoretical and from the experimental standpoint, the extent to which—in spite of its theoretical limitations—the method may still be utilised for the detection of hydrates in solution.

It is obvious from everyday experience of phenomena such as the efflorescence of hydrated crystals of sodium sulphate or of sodium carbonate, that the stability of a hydrate decreases as it becomes more and more complex. This general view is confirmed in detail by measurements of vapour-pressure, such as those recorded above for the hydrates of copper sulphate, and also by the thermo-chemical data. It is evident, for example, that although the hydrate $\text{SO}_3 \cdot \text{H}_2\text{O}$, in the formation of which 19,400 calories are liberated, may be sufficiently stable to melt without dissociation, this is less likely to be true of the hydrate $\text{SO}_3 \cdot 2\text{H}_2\text{O}$, in the formation of which an additional 6,400 calories only are liberated. The hydrates $\text{SO}_3 \cdot 3\text{H}_2\text{O}$ and $\text{SO}_3 \cdot 5\text{H}_2\text{O}$, which have also been isolated in a crystalline state, are probably still less stable compounds since the additional heat liberated in their formation amounts to only 3,040 and 2,700 calories. Whilst, therefore, the formation of the monohydrate and dihydrate may give rise to more or less abrupt changes in the physical properties of the solution, the effects produced by the formation of the more complex hydrates are likely to be spread over such a range of concentration as to be recognised only with difficulty. In the extreme case of the supposed hydrate with $5182\text{H}_2\text{O}$, the heat of formation amounts only to three or four calories per

molecule of water; the compound must therefore be excessively unstable, and it is hardly to be wondered at that the formulation of such a complex has come to be regarded almost as a *reductio ad absurdum* of the hydrate theory. Similar considerations have led to the non-recognition of hydrates with 51, 131, 508, and 1,600 molecules of water; the prevailing feeling appears to be that such compounds, even if present in the solution, would be so largely dissociated as to be incapable of producing any abrupt change in the properties of the liquid; the small irregularities on the strength of which these hydrates have been postulated appear to have been regarded as due to experimental errors, the effects of which would be most pronounced in dilute solutions.

Similar conclusions follow from a consideration of the experimental data. The best justification of Mendeleef's method of analysis is to be found in the study of the simple hydrates $\text{SO}_3 \cdot \frac{1}{2}\text{H}_2\text{O}$, $\text{SO}_3 \cdot \text{H}_2\text{O}$, and $\text{SO}_3 \cdot 2\text{H}_2\text{O}$, whilst its limitations are most clearly seen in the case of the more dilute solutions. Of the above compounds the two former are so stable that they are not usually considered as hydrates at all, but are referred to as "pyrosulphuric" and "sulphuric" acids respectively, although the acid containing 100 per cent. H_2SO_4 is known commercially as "monohydrate." The dihydrate of sulphur trioxide is less stable, but produces such marked effects in the solutions that its existence is scarcely a matter of controversy.

The conductivity curves are those in which the presence of the three hydrates is most clearly indicated. Water and sulphur trioxide are both excellent insulators when pure, but a mixture of one part of sulphur trioxide with four parts of water is one of the best electrolytic conductors known, the specific conductivity rising at this concentration to 0.7388 reciprocal ohms per centimetre cube at 18° C. The curve is, however, smoothly rounded, and since in the case of electrolytes it is usual for mixtures to show a higher conductivity than pure compounds, there is no reason for attributing the maximum to the formation of a hydrate, although the concentration at which it occurs would agree very fairly with that of a hydrate of the formula $\text{SO}_3 \cdot 18\text{H}_2\text{O}$.

In the case of the converse mixture of 4 parts of sulphur trioxide with 1 part of water precisely opposite conditions prevail. The conductivity falls at 81.44 per cent. SO_3 to 0.0080 ohm cm.⁻¹—a minimum value which is barely 1 per

cent. of the maximum recorded above. The minimum is, moreover, extraordinarily sharply defined, the conductivity being increased by 100 per cent. when the solution is mixed with 0.17 per cent. H_2O or 0.23 per cent. SO_3 only. According to W. Kohlrausch (*Wied. Ann.* 1882, 17, 69), the minimum is reached when the two substances are present in the molecular ratio $\text{SO}_3 : \text{H}_2\text{O} = 0.9975 : 1$, a value which differs so little from unity that the small deviation is probably within the limits of analytical error. At the same concentration the temperature-co-efficient of conductivity reaches a maximum at 4 per cent. per degree; this has the effect of making the minimum less sharp when the temperature is raised, but of accentuating it when the solution is cooled. It is an obvious inference from these observations that the hydrate $\text{SO}_3 \cdot \text{H}_2\text{O}$ is probably an insulator like the simple compounds from which it is derived; the fused hydrate is, however, dissociated to a small extent even at the melting-point, and the slight conductivity which it possesses may reasonably be attributed to its admixture with the products of dissociation; as the temperature is raised the dissociation increases, and as a consequence the minimum conductivity, which serves so emphatically to direct attention to the existence of the hydrate, gradually becomes smoothed out.

The half-hydrate $2\text{SO}_3 \cdot \text{H}_2\text{O}$ has an even lower conductivity, 0.0008 ohm cm^{-1} , than the monohydrate; but as mixtures of this acid with an excess of sulphur trioxide are also insulators, the minimum is less striking; the curve merely drops to the axis of zero conductivity and remains there until it reaches the end of the diagram.

The minimum at 69 per cent. SO_3 , corresponding with the composition of the dihydrate $\text{SO}_3 \cdot 2\text{H}_2\text{O}$, is of much greater interest, especially in the contrast which it presents with that due to the monohydrate. The actual conductivity of the liquid dihydrate at 18° is 0.0980 ohm cm^{-1} —a value twelve times as great as in the case of the monohydrate; and in order to increase the conductivity by only 3 per cent. it is necessary to add $2\frac{1}{2}$ per cent. of SO_3 or H_2O , the minimum being, therefore, 300 times less sharp than in the previous case. The contrast between the two compounds brings out in a very remarkable way the extreme stability of the monohydrate, and it is evident that in this case at least the conditions necessary for the successful application of Mendeleef's method are likely to be amply fulfilled.

That this is so may further be seen by referring to the densities of this series of solutions (Knietsch, *Ber.* 1901, 33, 4102). The formation of the monohydrate is marked by a remarkable break in the density curve for 15° C. which falls to a sharply defined minimum at 81.14 per cent. SO_3 (theory = 81.63 per cent.); this is shown on a large scale in the figure, from which it will be seen that the change of direction is almost as abrupt as that in the conductivity curve. Although, therefore it may be true from the standpoint of strict theory that "no homogeneous liquid mixture can show a discontinuous change

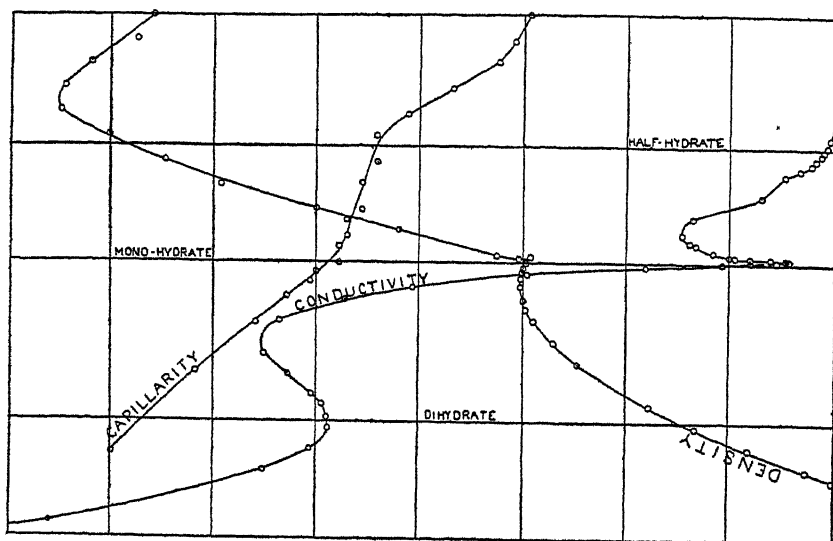


FIG. 1.—Diagram showing the properties of concentrated solutions of sulphur trioxide in water. The densities are from data by Pickering and by Knietsch, the conductivities are after F. and W. Kohlrausch, and the capillarities are those given by Knietsch; the viscosity data are too incomplete to be represented by a smooth curve and have been omitted from the diagram.

The concentrations shown are from 60 to 100 per cent. SO_3 ; the physical properties are plotted on arbitrary scales.

of density with composition whilst its temperature and pressure remain constant" (H. Hartley, *loc. cit.* p. 542), there are evidently instances in which a very high degree of experimental refinement would be required to demonstrate the continuity of the curve, whilst the "break" is so obvious as to render it extremely easy to locate its position, and almost impossible to overlook the existence of the hydrate by which is produced.

The half-hydrate $\text{SO}_3 \cdot \frac{1}{2}\text{H}_2\text{O}$, in spite of its great stability, does not reveal itself by any abrupt change in the density curve, but produces a remarkable break in the capillarity curve for 22°C ., when the hydrate is studied as an over-cooled liquid; the viscosity curve for the over-cooled liquid also reveals the presence of the half-hydrate by rising to a maximum of altogether exceptional sharpness.

The less stable dihydrate, on the other hand, produces no marked inflection of the density or capillarity curves, and (apart from the conductivity) is only revealed to the eye by an ill-defined maximum in the viscosity curve at 87 per cent. H_2SO_4 —theory 84.48 per cent.

In the case of the more dilute solutions no evidence of the formation of hydrates can be obtained by direct observation of the experimental curves; the only hope of detecting their presence in the solution consists in recording the slight changes of curvature which may be revealed by Mendeleef's method of differentiation or by the use of a bent lath. The difficulties associated with the application of these methods have already been referred to; but it will be appropriate at this point to state specifically that, even after differentiation, most of the curves appear to be reasonably smooth, and that an observer who was not on the alert to detect discontinuities would probably be content to represent even the derived experimental points by continuous rather than by broken curves. Mendeleef's anticipation that the examination of a single curve, such as the density curve for sulphuric acid, would reveal all the hydrates that might be formed in the solution has therefore proved to be unfounded in practice as well as in theory.

Later workers have been driven to base their conclusions in reference to the existence of hydrate in solution on "cumulative evidence derived from independent sources" rather than on the results obtained from the study of any one property of the solution.¹ This involves a vast amount of work, and the only case that has been adequately studied is that of sulphuric acid,

¹ "From the study of any one, or any few, particular breaks I concluded—*nothing*; from a study of a whole series of density results I only concluded that it was advisable to make other series at other temperatures; from the study of the series at four different temperatures I concluded only that I had 'strong presumptive evidence' of the existence of changes, but that confirmatory evidence

in which thirteen breaks have been detected at concentrations ranging from 2 to 98 per cent. H_2SO_4 . It is noteworthy that, in spite of the well-recognised instability of complex hydrates, the cumulative evidence for breaks at 3.99 and 9.69 per cent. H_2SO_4 (recognised in five properties) is greater than in the case of the seven breaks between 10 per cent. and 80 per cent. H_2SO_4 (recognised in three properties), and is actually greater than in the case of the hydrate $\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$, the presence of which was recognised in four properties of the solution.

As the modern theory of solutions does not admit of the recognition of hydrates of sufficient complexity to correspond in composition with the breaks that have been recorded for dilute solutions, it has been customary to pass them over as if they were inadequately established. This involves, however, a refusal to recognise the whole of the breaks between 0 and 80 per cent. H_2SO_4 , even if indeed it is permissible to select for recognition the break at 84.5 per cent. (corresponding with the hydrate $\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$), which is hardly more pronounced than those recorded at the lower concentrations. This refusal might be justified in the case of breaks deduced from the study of any one curve or property, but it is less easy to override the cumulative evidence which Pickering has put forward in this particular case. It appears, therefore, to be worth while to consider whether an alternative explanation cannot be devised which will admit of the recognition of at least some of the breaks without postulating the existence of an improbable series of unknown hydrates.

A possible clue to an alternative explanation is afforded by Rücker's analysis of the sulphuric acid density curves (*Phil. Mag.* 1891, 32, 304). He found that, between 46.9 per cent. and 80.5 per cent. H_2SO_4 , the relationship between density and concentration might be represented approximately by the equation

$$s = a + bx - cd^x,$$

but that there was a small deviation of the calculated from the experimental values, which reached a maximum at about 60 per

from the study of independent properties was necessary before such changes could be regarded as established; and it was only after obtaining such evidence from the study of three or four other properties that I ventured to call this evidence proof, and then only with the oft-repeated caution 'that many of these changes were admittedly of a very doubtful nature' (Pickering, *Phil. Mag.* 1892, 33, p. 143).

cent. H_2SO_4 . This deviation could be eliminated by grafting on to the original formula the expression

$$\frac{m}{72^x + 12^{-x}}$$

which raised the calculated value

by 0.000055 at 61.01 per cent.,

by 0.000010 at 52.91 per cent. and 69.10 per cent.,

by 0.000002 at 46.94 per cent. and 74.98 per cent.,

and by inappreciable amounts at higher or lower concentrations. When this addition was made to the original equation it agreed in a highly satisfactory manner with the experimental observations over the whole range, covering the supposed breaks at 51 per cent., 58 per cent., 72.8 per cent., and 78 per cent. Pickering, commenting on this equation (*Phil. Mag.* 1892, **33**, 139), called attention to the fact that the correcting factor in Rücker's formula reached a "practically appreciable" magnitude (0.000004) at 72 per cent., and at 50 per cent. corresponding closely with the positions of two of the breaks which he had postulated, and thus affording additional evidence in favour of his "principal contention"—the "practical starting of a fresh order of things at certain definite points."

So far as I am aware, it has invariably been assumed that this "fresh order of things" must be one resulting from the formation of a hydrate corresponding in composition with the concentration at which the break occurs. Pickering's contention certainly contains, however, at least the rudiments of an alternative explanation of the breaks. It may be supposed that the bulk of the solution consists, at dilutions below 80 per cent., of a welter of hydrates of small stability, and therefore so highly dissociated as to give a smooth density curve such as that represented by Rücker's original equation. Into this complex mass of hydrates it may be imagined that the pentahydrate $\text{SO}_3 \cdot 5\text{H}_2\text{O}$ is introduced as an aggregate of greater stability and somewhat higher density than the hydrates with slightly larger or slightly smaller proportions of water. The effect would be to increase the density to an extent depending on the proportion of pentahydrate in the solution—*i.e.* to a maximum extent at 58 per cent. H_2SO_4 (corresponding with the formula $\text{H}_2\text{SO}_4 \cdot 4\text{H}_2\text{O}$ of the hydrate) and to a progressively smaller extent at higher and lower concentrations. There would

thus be grafted on to the original smooth density curve a "hump" precisely similar to that represented by the correcting factor of Rücker's equation. *Pickering's conception, if followed to its logical sequel, leads therefore to the conclusion that the formation of the pentahydrate of sulphuric anhydride may be responsible, not only for the break at 59 per cent. H_2SO_4 , corresponding with the composition of the hydrate, but also for the adjacent breaks at 50 per cent. and 72 per cent. H_2SO_4 . If the breaks at 78 per cent. and at 92 per cent. H_2SO_4 are attributed in a similar way to the hydrate $\text{SO}_3 \cdot 2\text{H}_2\text{O}$, which produces the break at 84 per cent. H_2SO_4 , it is possible to account for the whole of the six breaks between 50 per cent. and 92 per cent. H_2SO_4 in terms of the hydrates $\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$, $\text{H}_2\text{SO}_4 \cdot 2\text{H}_2\text{O}$, and $\text{H}_2\text{SO}_4 \cdot 4\text{H}_2\text{O}$, which have actually been crystallised out from the solutions.*

If the breaks at 19 per cent., 30 per cent., and 38 per cent. H_2SO_4 are attributed in a similar manner to the formation of an unknown hydrate containing a larger proportion of water, it is possible to account for all the breaks from 10 per cent. to 90 per cent. H_2SO_4 by assuming the existence of three known and one unknown hydrates, in place of the three known and six unknown hydrates postulated by Pickering.

The breaks at 3.99 and 9.69 per cent. H_2SO_4 demand separate consideration. They cannot be accounted for by the formation of hydrates, since compounds such as $\text{SO}_3 \cdot 52\text{H}_2\text{O}$ or $\text{SO}_3 \cdot 132\text{H}_2\text{O}$ could scarcely be formed in quantities sufficient to disturb the course of the density curve. Nevertheless the most casual inspection of the curve of the first differentials reveals the fact that the most violent twist occurs at a concentration of less than 5 per cent. It is therefore natural to attribute it to alterations in the properties of the solvent, the complex character of which is now well recognised. The presence in liquid water of two kinds of molecules—a dense "water"-molecule and a lighter though more complex "ice"-molecule—was postulated by Röntgen (*Wied. Ann.* 1891, 45, 91) in order to account for the contraction which takes place when water is heated from 0° to 4° , and Sutherland, who has elaborated this view, has suggested (*Phil. Mag.* 1900, 50, 460) that the ice-molecules may consist of trihydrol, H_6O_3 , and the water-molecules of dihydrol, H_4O_2 . The presence of a third kind of molecule of simpler structure and lower density than the typical water-molecule has also been suggested (Bousfield and Lowry, *Phil. Trans.* 1905, 204, 283) as affording

an explanation of certain observations in connection with the density of aqueous solutions of sodium hydroxide. The addition of a solute has the effect of eliminating from the solvent water the more complex molecules, so that even in comparatively dilute solutions the maximum density can no longer be observed. There can be little doubt that the abnormal course of the density-concentration curves is due to the same causes, and that *the breaks recorded for dilute solutions of sulphuric acid are due, not to the formation of hydrates, but to the recovery of the density curve from the distortion produced by alterations in the character of the complex water-molecules.*

Similar considerations may be applied in the case of the most concentrated solutions, the changes at 97.4 and 93.9 per cent. H_2SO_4 being attributed to the elimination of complex acid molecules of the type $(\text{H}_2\text{SO}_4)_x$ and not to the production of hydrates with one-sixth, one-third, etc., molecules of water. The net result would then be that the whole of the thirteen breaks between 0 to 100 per cent. H_2SO_4 can be accounted for by assuming the existence of four hydrates, together with a certain number of changes due to depolymerisation of the water and acid. These assumptions are fully in accord with present-day conceptions of the nature of solutions, and have the advantage of eliminating most of the difficulties that have hitherto been felt in recognising the actual existence of Pickering's breaks.

It will be of interest, as confirming the accuracy of these conclusions, to refer briefly to one of the few cases which have been studied with something like the care which was bestowed on the investigation of sulphuric acid. In the case of caustic soda the hydrates which separate on cooling the solutions include compounds with 1, 2, 3.5, 4, 5, and 7 H_2O (Pickering, *Trans. Chem. Soc.* 1893, **63**, 890), and breaks corresponding with most of these were detected in the somewhat complex density curve (*Phil. Mag.* 1894, **37**, 359). In addition to these, however, breaks were detected at 20 per cent, 12.1 per cent. and 3-4 per cent. NaOH , corresponding with the formulæ $\text{NaOH} \cdot 9\text{H}_2\text{O}$, $\text{NaOH} \cdot 16\text{H}_2\text{O}$, and $\text{NaOH} \cdot 53\text{H}_2\text{O}$. In this case a later series of independent density measurements is available (Bousfield and Lowry, *Phil. Trans.* 1905, **204**, 265-73), from which it appears that the degree of accuracy attained in the observations in which the breaks were detected was considerably over-estimated; the absolute error in the concentration of the 50 per cent. stock solution, estimated at

0.05 per cent., was probably three times as great, and even after allowing for this the two series of observations differ at 25 per cent. to the extent of 0.0004, in spite of the fact that the experimental error was estimated at 0.00000631 only. Greater interest attaches, however, to the fact that from 5 per cent. to 18 per cent. there is an exact linear relationship between the density and concentration, the maximum error amounting to only two units in the fifth place of decimals; it is therefore evident that Pickering's break at 12.1 per cent. must have been due either to unsuspected experimental errors similar to those referred to above, or to the inability of the bent lath to follow the changes of curvature in the derived curve which he selected for examination.

If produced to zero-concentration the linear part of the density curve would cut the axis of density at 0.99939 instead of at 0.99866—the actual density of water at 18°. There is, therefore, considerable bending in the region from 5 to 0 per cent., which includes Pickering's break at $\text{NaOH} \cdot 53\text{H}_2\text{O}$. That this sudden twist is due to the depolymerisation of the complex water-molecules by the caustic soda, and not to the formation of a new hydrate, is abundantly proved by a study of the density-temperature curves (Bousfield and Lowry, *loc. cit.* pp. 274–80). The maximum density, which forms the chief evidence of the presence of the ice molecules, is lowered to 0° by the addition of 0.6 per cent. NaOH, and to –10° by 2 per cent. NaOH only; at 12 per cent. NaOH the solution has become so far simplified that the effect of temperature can be represented by a quadratic instead of a cubic equation. The density-temperature curves for the concentrated solutions are practically straight, but on dilution steadily approximate to the complex curve which represents the expansion of water. It is therefore impossible to avoid the conclusion that the complexities of the dilute solutions are due to the inherent properties of the solvent and not to those of the solute.

The new interpretation of Pickering's breaks, which is described above, differs considerably from that which he himself advocated, but not more widely than his breaks differ from those which were sought and found by Mendeleef. The latter looked for sudden changes of direction and curvature similar to those which actually occur in the case of the hydrate $\text{SO}_3 \cdot \text{H}_2\text{O}$; such changes can only occur in the case

of hydrates of exceptional stability, and must appear at concentrations identical with those of the solid hydrates. Pickering, on the other hand, looked for minute alterations in the laws which govern the relationship between curvature and concentration, and by applying methods of exceptional refinement was enabled to detect discontinuities of an entirely different order. These might be produced by a great variety of causes and need not occur at exact molecular compositions. But such was the dominating influence which had been acquired by Mendeleef's conception that, even up to the present time, no alternative explanation appears to have been forthcoming, except the hackneyed criticism that the breaks were due to imagination.

THE SPIROCHÆTES:

A REVIEW OF SOME BORDER-LINE ORGANISMS BETWEEN ANIMALS AND PLANTS

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AMONG the many micro-organisms now receiving attention from both medical and biological investigators, few are of greater interest than the Spirochætes. They are long, wavy, thread-like organisms, lying on the border-line between animals and plants, and are variously regarded as Bacteria or Protozoa. It is, perhaps, still difficult to settle their systematic position definitely, and, indeed, it is to some extent unnecessary, for Haeckel long ago gave the name *Protista* to the lowliest organisms, whether animal or plant, the division between which is not hard and fast.

Spirochætes are of great economic importance, for it is to these organisms that the pathogenic agents of relapsing fever in human beings and fowls belong, also of tick fever in man, and probably of syphilis (fig. 1, c).

Spirochæta is the name given by Ehrenberg in 1833 to a genus of microscopic, sinuous, thread-like forms which are relatively of great length compared with their breadth (fig. 2, B). The type species, *S. plicatilis*, was found in pond-water by Ehrenberg; and was described and figured by him in a paper (4)¹ published in 1835. According to the great protistologist, the late Dr. Fritz Schaudinn (10), this type species possesses an undulating membrane (fig. 1, A). Strict members of the genus *Spirochæta*, then, should possess an undulating membrane or lateral extension of the outer layer (ectoplasm) of the body. This membrane is an outgrowth and is wound spirally round the body, forming, as it were, a spirally coiled lateral fin, which is used in locomotion (figs. 2, A, B).

¹ The numbers in parentheses refer to the List of References to Literature at the end of this memoir.

On the other hand, members of the genus *Spirillum* are characterised by the possession of fine protoplasmic processes (cilia or flagella) as outgrowths of the body, and the latter is rather more rigid and so less sinuous than that of a Spirochæte.

Unfortunately the generic and specific characters of these and of related forms are in a most confused state, and in some memoirs by well-known workers mention is made of the "Spirochætes of Spirillosis."

The general shape of a Spirochæte is, as previously mentioned, that of a long, narrow, wavy thread, which is at once suggestive of a bacterial organism. Some Spirochætes, however, are very large for Bacteria. The body is bounded externally by a definite, but flexible and very thin layer, which has received the name of periplast.

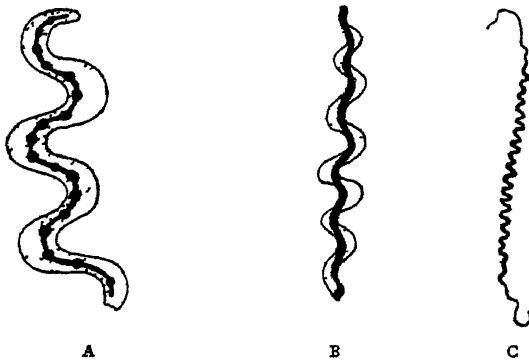


FIG. 1. (A) *Spirochæta plicatilis* (Ehrenberg), end portion of a long individual. The central axis (black) is the nucleus, and is surrounded by the endoplasm and the ectoplasmic undulating membrane. In this figure (after Schaudinn) the ectoplasmic and endoplasmic structures are not clearly differentiated from each other.

(B) *Spirochæta refringens*, after Schaudinn.

(C) *Treponema pallidum* (*Spirochæta pallida*), after Schaudinn.

The possession of a specially differentiated portion of protoplasm, known as the nucleus, is characteristic of most unicellular organisms, especially animal ones. The chief constituent of this nucleus is a substance known as chromatin, on account of its capacity for taking up various stains and being coloured thereby.

Usually the nucleus is condensed, and so is well marked after staining. Its position in the cell is often central. The principal exception to this is found in the Bacteria, which

have been asserted by some to be devoid of a nucleus, though this is incorrect, for many Bacteria possess a diffuse nucleus of scattered granules of chromatin. In the Spirochætes, as shown by recent researches, there is a spiral of chromatin (fig. 2, c, d) running practically the whole length of the organism (5, 6, 12). Such a distribution of chromatin represents a diffuse nucleus. One of the largest of the Spirochætes is *S. balbianii* (fig. 2, A),¹ from the stomach, intestine, and crystalline style of the oyster. Another species, *S. anodontæ*, occurs similarly in the alimentary tract of the pond-mussel (fig. 2, B).

The presence of a spirally wound and undulating membrane has already been mentioned. In *S. balbianii* this possesses a thickened border (fig. 2, A) which colours deeply with chromatin stains and gives the general reactions of the substance chromatin. Such a chromatic border is probably a constant feature of Spirochætes, but is not easily seen in the smaller species.

It is most instructive, though difficult, to follow the movements of Spirochætes under the microscope. This study of movements is not only interesting, but is of economic importance; for, as I hope to show in the sequel, it should be possible to determine from its movements whether an organism belongs to the genus *Spirillum* or *Spirochæta*.

Researches of this nature require the examination of living material, which tends to be very much overlooked or even entirely ignored. It is not sufficient to rely on stained preparations alone. For the observation of the living Spirochæte, care must be taken to place it in its natural environment as far as possible. It is useless to record the movements of a parasite under unfavourable conditions, such as when its host begins to react upon it.

The movement of Spirochætes has only been investigated to any extent in the case of *S. balbianii* and *S. anodontæ* (5). Each of these moves very rapidly, especially *S. anodontæ*, with its pointed ends (fig. 2, B). It is almost impossible to analyse their motion when moving at full speed. In the case of slowly moving specimens, it is seen that the Spirochæte moves forwards while rotating on its long axis. The movement appears to be resolvable into at least two elements or components: (i) a wave-like flexion of the body chiefly for

¹ First described by Certes as *Trypanosoma balbianii* in 1882.



FIG. 2. (A) *Spirochata balbianii* (Certes). The membrane (*memb.*) with its nearly longitudinally arranged myoneme fibrils and thickened chromatic border (*chr. border*) is clearly indicated. A basal granule (*b.g.*) occurs at each end of the parasite, and the transverse bars in the body are composed of chromatin and are portions of the nuclear apparatus. Some myoneme fibrils (*my.*) are also seen on the periplast of the body. (After Fantham [5].)

(B) *Spirochata anodontae* (Keysselitz), showing numerous sinuosities in the outline of the body and pointed ends with basal granules (*b.g.*). The spiral winding of the membrane is clearly shown, also the transversely arranged chromatin rodlets. (After Fantham.)

(C) *Spirochata balbianii*, early division stage. The membrane has divided longitudinally, and each portion of the divided membrane has its own set of myoneme fibrils. The chromatin is in the form of a spiral or helix, especially well-marked in the middle portion of the body. Two basal granules, indicating division, occur at each end. (After Fantham.)

(D) *Spirochata balbianii*, showing longitudinal fission, as in the last figure, but the approximately longitudinal arrangement of the myonemes is well shown. (After Fantham.)

(E) *Spirochata balbianii*, late division stage, the daughter forms being still attached at one end. (After Fantham.)

progression, and (ii) a corkscrew motion of the body as a whole, due to the spiral winding of the membrane. Forms with pointed ends, like *S. anodontæ*, exhibit the corkscrew motion very well, and easily bore their way through the débris of cells floating in the gut contents of the host.

The figures of *S. plicatilis* of Ehrenberg strongly suggest that it possesses a spiral motion, and small forms which I have seen in pond-water, and which may be *S. plicatilis*, support this view.

While the organism is moving, waves can be seen travelling down the thread-like body in a direction opposite to that in which the organism is progressing. Many such waves are seen passing down the body of rapidly moving forms, while few (some two to four) waves occur in more slowly moving ones. Very rapidly moving specimens appear almost straight, each of the many undulations of the body being only slight.

It is also interesting to observe that the movements occur in jerks. A Spirochæte may suddenly come to a dead stop, or just as suddenly proceed to move slowly. It does not seem to matter in the least which end of the body is directed forwards, for the organism is able suddenly to reverse its direction of movement and to return on its own path, whether it be straight line or circle. A Spirochæte, then, can move with or against the current indifferently, and much energy seems to be used in movement, though without very adverse effect on the normal organism. It has been suggested by Novy in the case of *S. obermeieri* that the retracing of the path is due to unfavourable environment. This is, I think, doubtful, if not incorrect, for I have observed both *S. anodontæ* and *S. balbianii* constantly retracing their path in the crystalline style of their bivalve hosts.

When the Spirochæte appears suddenly from the deeper levels of the liquid under examination, it is seen to bore its way more or less vertically upwards, twisting itself into various complex shapes, forming very beautiful figures such as catherine wheels, but sometimes almost bewildering to watch or to analyse. If one end of such a parasite be anchored to débris, the free end is seen to execute violent lashing movements. Slowly moving specimens frequently curl up one end. To sum up as regards movements, Spirochætes seem to move more quickly than Trypanosomes, and with an added corkscrew motion. Their

progression may be compared with that of an eel, with the spiral movement in addition.

The structure of the membrane must now be mentioned in some detail, and its relation to the mechanism of movement of the organism. After appropriate staining, as by gentian violet, iron hæmatoxylin, or Delafield's hæmatoxylin, this structure is seen to be fibrillar in *S. balbianii* (fig. 2, A, c, D) and *S. anodontæ*. Modifications of the Romanowsky stain do not show this fibrillar structure so clearly. Allowing for the spiral, sinuous character of the membrane and parent organism, it is seen that these fibrils are almost longitudinally arranged (fig. 2, A, D). It is necessary to consider carefully the sinuities and foldings of the membrane in determining the direction of the fibrils, as they may sometimes appear somewhat transverse (fig. 2, c). The fibrils

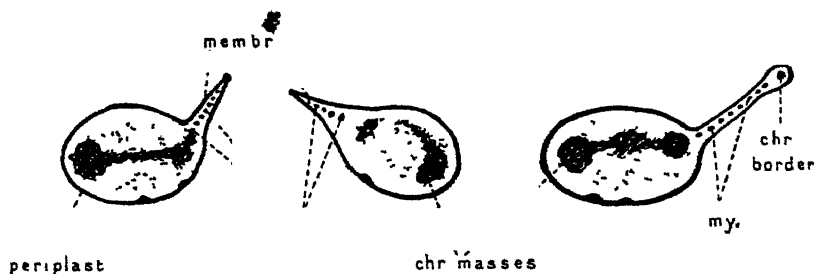


FIG. 3.—Diagrams of transverse sections of Spirochætes from an infected Lamellibranch crystalline style. The membrane (*membr.*) is seen to be a lateral extension of the periplast containing myonemes (*my*), and a chromatic border. The membrane is spirally wound, for a node of the membrane is passed through in *c*. (After Fantham.)

are termed myonemes, and some of them also occur in the periplast of the body of the organism. Two sets of myonemes are seen in dividing forms (fig. 2, c, D). When the membrane is closely contracted against the body, it may appear like a sheath around it, as was once described for *S. balbianii*. However, it is not a sheath, but a lateral outgrowth of the ectoplasmic periplast, as is seen from a consideration of fig. 3. When loose, the membrane is clearly seen to be spirally wound round the body of the organisms (fig. 2, A, B, C, D), for its chromatic border can be discerned in the troughs of the wave-like body even under comparatively low powers. It is erroneous to state that specimens of *S. balbianii* may occur devoid of a membrane. Such specimens really possess a closely contracted membrane.

Probably the undulating movements are principally due to the myonemes of the body surface, while ordinary progression is dominated by those of the membrane.

Internal to the cuticle or periplast is an almost homogeneous protoplasm, in which an inner, slightly more granular endoplasm sometimes can be distinguished from an outer ectoplasm. There is not a condensed nucleus as occurs in most Protozoa and in the Metazoan cell, but the chromatin is distributed—in other words, the nucleus is diffuse, an important fact first pointed out by Laveran and Mesnil in *S. balbianii*. In well-fixed and stained preparations of *S. balbianii*, the chromatin is seen to be arranged in transverse bars at more or less regular intervals (fig. 2, A). In the most successful preparations, a connecting

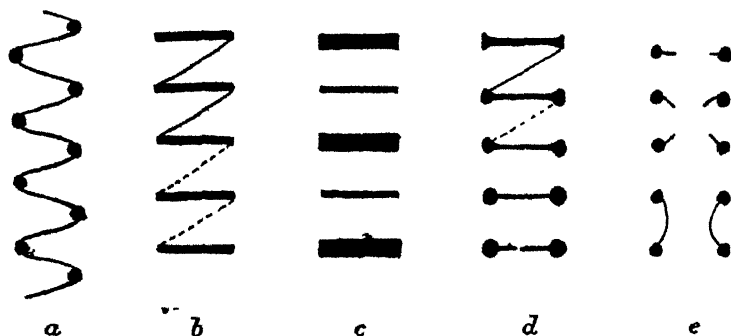


FIG. 4.—Diagrams of the nuclear helix as seen in Spirochaetes. *a*. Loose helix, with chromatin masses at the turns, connected together chiefly by achromatic substance. *b*. Shows scalariform rodlets of chromatin with slight amount of achromatic substance. *c*. Shows rodlets of chromatin, some thicker than others, because they were originally arranged on a helix, and so were at different depths of focus. *d*. Shows arrangement of the rodlets into dumb-bell shaped chromatin masses. *e*. Shows the chromatin in longitudinal division of an organism, where the dumb-bell shaped chromatin masses break across, and the halves contract towards the periphery, while the remains of the chromatin threads "loop up" to form the chromatin apparatus of the daughter organisms. (After Fantham.)

spiral or fine zigzag filament is seen between the bars (figs. 2, c, d; 4, a, b, c, d, e). The filament does not stain so deeply as the bars, and contains much achromatic substance.

During division the chromatic spiral divides longitudinally in the manner indicated in fig. 4 (*a, b, c, d, e*), each transverse bar becoming dumb-bell shaped, and breaking across in the middle.

In *S. balbianii* there is a small cap or nodule at each end of the periplast, and a dot of a similar nature at the ends of *S. anodontæ* (fig. 2, A, B, C, D), which stains red with Giemsa's solution and gives the chromatin reaction with hæmatoxylin or gentian violet. Such has been called a basal granule (fig. 2, A; *b.g.*) and it consists of chromatin-like substance from which in *S. anodontæ* (fig. 2, B) arises a short, stiff process which is not motile, and so is not a flagellum.

Spirochætes divide longitudinally, that is, in the direction of their long axis. In this respect they resemble Trypanosomes and other flagellate Protozoa. Transverse fission is also known among these organisms, and has been definitely recorded in *S. balbianii* (5, 6), and *S. duttoni* (2, 3); a characteristic in which Spirochætes resemble most Bacteria.

The full details of longitudinal division have been described only recently, and that more especially in *S. balbianii* and *S. anodontæ*. The first structures or "organellæ" to show signs of division are the basal granules and membrane (fig. 2, C, D). The basal granule at each end divides, and a double membrane, each part with its myonemes, is seen. Breaking apart occurs at one end, and the free ends gradually diverge like the legs of a pair of compasses (fig. 2, E). Finally division is completed at the hitherto undivided end.

Encystment has been described by Perrin (6) in *S. balbianii*, and it has been suggested that the appearances seen in certain other Spirochætes may be thus explained; but the question of encystment in Spirochætes in general is doubted by some authors, as is also conjugation.

Similarly, it has been stated that gametes occur in *S. balbianii*, *S. gallinarum*, etc., narrow forms being regarded as males, wider forms as females. But these appearances can be otherwise interpreted, the male and female forms being merely the extremes in a more or less continuous series.

Having given now an account of the structure and the life-history, so far as is known, of a typical Spirochæte, we may consider broadly the position of Spirochætes in classification, and some points connected therewith. The present writer recently tried various tests to endeavour to determine the nature of the cuticle or periplast. This was very difficult to accomplish definitely, because of the smallness of the organism, the small quantity of material which was available, and the vague and

unsatisfactory nature of both the reagents and tests at present applicable. In the end, a chitinoid substance appeared to be present in the periplast and not fungus-cellulose. This is in favour of the protozoal or animal nature of Spirochætes.

This periplast is, however, quite flexible, more so than in the genus *Spirillum*. This question of rigidity occasionally has been used as a test of the protozoal or bacterial nature of a protist organism. Again, the consideration of cultures or preparations of artificial media in which Spirochætes might be reared is more or less concerned with the bacterial or other nature of these organisms. So far as I know, no successful medium capable of sustaining the life of any Spirochæte for any length of time has yet been found. This has been remarked by Novy, Perrin, the present writer, and others. Those Spirochætes on which the experiments have been tried are said to be implasmolysable.

It has been stated (3) that *S. duttoni* is a Protozoon, because regular, recurrent relapses of tick fever result from its presence in the system.

These points of difference, however, must only be treated broadly and not emphasised too much singly, for doubtless exceptions to some of them can be found.

We may now briefly summarise these and other relevant characteristics under two heads, taking first the principal characteristics that rather suggest the inclusion of the Spirochætes among the Bacteria :

(a) The diffuse condition of the nucleus, directly comparable with that of Spirilla and somewhat like that of *Bacillus bütschlii* (Schaudinn).

(b) The occurrence of transverse fission among Spirochætes.

(c) The absence of a blepharoplast in the form in which it is found in Trypanosomata.

As characteristics favouring the inclusion of Spirochætes in the Protozoa, we may note :

(a) The possession of a membrane.

(b) The occurrence of longitudinal fission.

(c) They are implasmolysable.

(d) The inability, up to the present, to find suitable artificial media in which Spirochætes may be kept alive.

(e) The presence of a small amount of chitinoid substance in the periplast and membrane.

(f) Bacteriologists comment on the large size of *S. balbianii*. But *S. anodontæ* is much smaller, and *S. obermeieri* is smaller still. Little importance is to be attached to size alone.

Prowazek (7) mentions that *S. gallinarum* may occur for a time inside a red blood corpuscle of the fowl, and that this characteristic is against the parasite being a Bacterium. I have seen *S. balbianii* and *S. anodontæ* penetrate epithelial cells of the gut wall of the Lamellibranch and be attached to shed epithelial cells in the alimentary tract. But these are more or less accidental, and there does not appear to be a true or definite intra-cellular stage in the life-history of any Spirochæte.

A few words may be written now concerning some of the pathogenic Spirochætes, though fuller accounts (*e.g.* Blanchard, 1) from the pathological side should be sought in various medical journals.

S. obermeieri, the pathogenic agent of European relapsing fever, is spread by bed bugs. This organism probably needs re-investigation, for, according to Novy, it belongs to the genus *Spirillum*. The species was founded by Cohn in 1875, having been first notified by Obermeier in 1873. It is from 15μ to 40μ in length, and pointed at both ends.

S. gallinarum, found in the blood of fowls, is the pathogenic agent of relapsing fever in those birds, and is spread by fowl ticks of the genus *Argas*. Borrel has described this organism as possessing lateral or peritrichal cilia, while Prowazek (7) describes it as an undoubted Spirochæte with an undulating membrane. Prowazek's account is probably the more correct, and the so-called cilia of Borrel are myoneme fibrils from a frayed or ruptured membrane. Vlès' observations (14) on *S. balbianii* are thus explained.

S. anserina occurs in the blood of geese.

S. duttoni is the pathogenic agent of tick fever, especially prevalent in the Congo State. It has been investigated by workers at the Liverpool School of Tropical Medicine (2). It is from 13μ to 43μ long. It is spread by ticks, and the Spirochætes find their way into the ova of ticks, thus infecting the progeny of the latter. Dutton and Todd (3) quite recently have described both longitudinal and transverse fission in this form (as in *S. balbianii*), and the presence of an undulating membrane, as suggested by the present writer (5, p. 34, footnote).

S. dentium occurs in the human mouth, and is very small.

S. muris (Wenyon) or *S. laverani* (Breinl and Kinghorn) is found in the blood of mice. It is a very small form.

S. refringens (Schaudinn) is associated with *Treponema pallidum* in syphilitic lesions. *S. refringens* is the more refractive in life, and has a smaller number of undulations than *T. pallidum*, while it stains more readily (fig. 1, B, C).

The accounts of the structure of many of these Spirochætes are incomplete. Most of them are very small in size, and their chromatin structure and details regarding a membrane usually need re-investigation.

S. culicis (Jaffé) has been described recently from the alimentary tract of the larva of a mosquito, *Culex pipiens*. Still more recently a species, *S. bufonis* (Dobell), has been notified from the rectum of a toad.

Spirochæta gigantea (Warming, 1875, from the coasts of Denmark), found in brackish water, is stated by Swellengrebel (12) to be a *Spirillum*, and recently has been described very carefully by him as *Spirillum giganteum*. It divides by transverse fission.

It will thus be seen that three main distributions are known for Spirochætes—namely, the blood stream, the various fluids of the alimentary tract, and water (e.g. pond-water for *S. plicatilis*). These media probably represent in inverse order the evolutionary line of habitat of these organisms.

The study of Spirochætes has attracted much attention during the last few years, since Schaudinn published his remarkable memoir on *Trypanosoma noctuæ* and *Trypanosoma ziemannii*, and their relations with the Hæmosporidia. The latter organism (*T. ziemannii*) forms Spirochæte-like threads by continued longitudinal fission, but these possess a condensed nucleus.

The same brilliant investigator, the late Dr. Schaudinn, in collaboration with Hoffmann (11), also found a spirochætiform organism in syphilitic lesions, which form was called *Spirochæta pallida* (10) at first. It is known now, more correctly, as *Treponema pallidum*, for it has not been shown to possess a true undulating membrane, and it is very long and thin, being only about 25μ broad and some 14μ in length. The spirals in it are preformed. The ends of the body are finely tapering, and these long, thread-like ends may be delicate, filamentous flagella (fig. 1, c). Krzysztalowicz and Siedlecki describe a clear, oval area in the

middle, which they regard as a nucleus, poor in chromatin. They also consider that this organism possesses a Trypanosome phase (*T. luis*), with a flagellar border and a "centrosome" granule, and they bring forward certain observations in support of the occurrence of sexual forms and conjugation. No internal, spiral, chromatin body has been described as yet in *Treponema pallidum*, and it does not appear to belong strictly to the genus *Spirochæta*. Many other short papers have been written concerning *Treponema pallidum*, but little of its life-history is known with certainty. It would therefore seem inadvisable, or at least premature, to discuss this organism any further.

S. pertenuis (Castellani), from "yaws" or "frambœsia," is another spirochætiform organism of doubtful systematic position.

On the whole, the protozoal characteristics of the Spirochætes seem to preponderate over the bacterial, and the present writer inclines to the view that they are lowly forms of Protozoa,¹ for which a new (fifth) class may have to be founded, when our knowledge of these very interesting organisms is fuller. At present one can only definitely state that they are Protists.

To summarise: the Spirochætes are characterised by a long, thread-like body, a membrane, the absence of true flagella, flexibility of the body, the ends of the body being either rounded or prolonged into a stiff process, the nucleus consisting of a spiral filament, the occurrence of longitudinal division, and the presence of a probable chitinoid substance in the boundary layer or periplast. These characters might be cited for distinguishing such a fifth (Spirochætacean) class of the Protozoa; but, considering the present state of our knowledge, one must not push matters. Classification only marks the state of knowledge for the time being, and arbitrary boundaries, marked off by hard and fast lines, are not usually found in nature.

Before concluding, a suggestion might be put forward as to the significance of the chromatin apparatus of the Spirochætes and its relation to the condensed form of nucleus found in most Protozoa and in the typical metazoan cell. Robertson (8),

¹ Since the above was written, Krzystalowicz and Siedlecki have published a further memoir on *Spirochæta pallida*, at the end of which they suggest that the Spirochætes should be placed in a distinct section of the *Mastigophora*, for which they propose the name *Spirilloflagellata* (vide *Bull. Acad. Sci. Cracovie*, 1907, No. 3, p. 231).

Salvin-Moore and Breinl (9), and Swellengrebel (13) have all described the presence of a filament, largely achromatic, in several species of pathogenic Trypanosomes (*T. brucei*, *T. gambiense*, and *T. equinum*). This filament has been variously called by these writers the "central line," the "black line," and the "achromatic filament" respectively. It has been seen independently by the present writer also in these species, and in addition in *T. evansi* of Surra.

Robertson has attempted briefly and ingeniously the homology of this line with portions of the condensed nucleus of cells of the higher forms, using some diagrams of hypothetical ancestral flagellates given by Perrin (6).

It might be urged that these appearances are due solely to degeneration, or to the reaction of the host on the parasite. In some of the cases described the infection certainly was an "artificial" one—e.g. *T. brucei* in a guinea-pig and *T. gambiense* in white mice. But even then, these appearances may be examples of "reversion." They cannot be due merely to faulty technique, for Moore and Breinl have taken the greatest care with their fixation and staining. Chromatin granules, more or less scattered, have long been notified in the general cytoplasm of Trypanosomes.

Again, in this connection, the moniliform character of the trophic nucleus of *Spirostomum* is worthy of note. Here we have a nucleus of a ciliate Protozoon with an undoubted likeness to the nuclear figure of *Spirochæta plicatilis*, according to Schaudinn (fig. 1, A.). So far as I know, this relationship has not been remarked on before, and it is, it seems to me, much more than a mere resemblance or convergence.

The spiral axis of the nuclear apparatus of Spirochætes seems to me to be a karyosome, largely achromatic, with at times some chromatin in the achromatic substance, which chromatin has migrated thither in the form of granules or chromidia. The chromatin is principally found as transverse rods arranged along the spiral, and giving the spiral a somewhat flattened appearance. In the concentrated nucleus of most Protozoa and the Metazoa, this spiral is either condensed in its entirety, or only the middle portion of the long helix of the Spirochæte remains, the rest aborting.

Probably the latter view is the more correct, judging by the number of chromosomes usually found in the cells. The oval

character of the nucleus of *T. brucei* with the transverse arrangement of its eight chromosomes as figured by Prowazek and Robertson supports this. The transverse bars of chromatin in *Spirochæta balbianii* are, then, probably comparable to chromosomes, as suggested by Perrin.

To summarise: first we have a diffuse nucleus of scattered chromidial granules as in *Bacillus bütschlii* and many rod-like Bacteria; next, the arrangement of these granules to form a spiral or helix, as in *Bacillus maxinus buccalis*, and more clearly in *Spirochæta balbianii*; lastly, we have the condensed nucleus of Trypanosomes and the cells of the Metazoa, the connecting link being supplied more especially by the work of Robertson and Swellengrebel.

This review is, perhaps, hardly the place to pursue this interesting hypothesis further. Suffice it to say that the study of the chromatin of Bacteria and Spirochætes suggests interesting chapters in the evolution of the metazoan nucleus.

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REVIEWS

The Origin of a Land Flora: A Theory based upon the Facts of Alternation. By F. O. BOWER, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. [Pp. 727, with 361 Figs. and Frontispiece.] (Macmillan & Co., London, 1908.)

A SHORT statement of the theory forming the thread which binds this volume together was published by the author in 1890.¹ Since that date his extensive series of researches on pteridophytes has appeared under the title of *Studies in the Morphology of Spore-producing Members*. When one bears in mind that these solid contributions to our knowledge of the vascular cryptogams represent preparatory investigations carried out with the object of supplying data for testing or elaborating the original theory, one may be prepared to some extent for the mine of information contained in the present work. The chief subjects coming under consideration are the characters of the sporophyte generation in all the groups of Pteridophyta, living and fossil, and in the Bryophyta; the author's own investigations and all other available sources being drawn upon to supply material for theoretical sifting.

The theory in question, called by the author "the biological theory of antithetic alternation," may be outlined as follows: in the land flora the striking constancy of alternation of generations is to be explained as the expression of an amphibial existence; the gametophyte is derived from aquatic ancestors (possibly Algæ), and remains aquatic as regards its motile gametes throughout the Archegoniata, while the sporophyte is the characteristically subaërial generation, the maturing and dispersal of its spores being normally connected with dryness. The importance of the sporophyte is ascribed to the fact that in these terrestrial organisms (Archegoniata) sexual reproduction (which is aquatic) will be precarious, hence increase in the number of individuals by another method suitable to drier conditions will become of great value. Antithetic alternation of generations, as distinguished by Celakovsky, is adopted as applying to the Bryophyta and Pteridophyta, *i.e.* the sporophyte generation is held to have been a phase intercalated between successive gametophytes, and its origin is referred to certain cell-divisions succeeding sexual fusion, such as occur in some of the lower plants, and are in some cases connected with the reduction of the chromosomes. The sporophyte generation at its initiation is thus pictured as a mass of spores resembling the group of reproductive cells formed from the zygote in some Algæ. From this lowly beginning the sporophyte is held to have advanced to gradually increasing complexity by means of various modifications, all of which are shown to be intelligible as conferring a biological advantage, such as sterilisation of sporogenous cells to supply protective tissue for enclosing the spores, and a vegetative system for their nutrition; the segregation of the sporogenous tissue into distinct pockets or sporangia; the origin of appendicular organs serving a variety of purposes.

¹ "On Antithetic as distinct from Homologous Alternation of Generations in Plants," *Annals of Botany*, vol. iv. p. 347.

These are some of the fundamental points in the theory, which is submitted by the author as a working hypothesis, since the supposition that the alternation of generations, shown by bryophytes and pteridophytes, really is antithetic in origin, appears to lie outside the realm of possible proof under present conditions.

The book is divided into three parts, of which the first is devoted to a statement of the working hypothesis. Here various subjects are dealt with in turn ; in each chapter an excellent summary is given of the present state of knowledge with regard to one subject, and the facts elicited are then discussed from the point of view of their bearing on the biological theory of alternation. Among the subjects discussed in this part of the book are the cytological distinction of the alternating generations ; apogamy and apospory ; the morphology of sporangiophores and sporophylls ; sterilisation ; embryology and the theory of recapitulation ; vascular anatomy.

The second part of the book contains a detailed description of the sporophyte in the different groups of Bryophyta and Pteridophyta, taken serially, and treated intimately as regards the form, anatomy, and development of the vegetative and reproductive parts. In the different phyla careful comparisons of series of allied forms are entered into for the purpose of obtaining views on their phylogenetic sequence, the morphological status of their organs, etc.

The third part is of the nature of a summary of the preceding portion of the volume, treated in relation to the working hypothesis, and as a source of some general phyletic and morphological conclusions. Besides other subjects this part contains a comparative description of the embryogeny of the pteridophytes, and a chapter on heterospory and the seed-habit in their biological aspect.

A few conclusions regarding the Pteridophyta may be quoted here. In the early history of the sporophyte the axis was pre-existent to its appendages, which subsequently arose by enation from its surface, and probably independently in different phyla. Foliage-leaves have originated from sporophylls or bracts, the sporangia or other spore-bearing organs which they bore or subtended having disappeared ; this belongs to the theory of the strobilus. Sporangiphores are parts *sui generis*, and are not the result of modification or replacement of any other sort of appendage. The primitive type of sporophyte for the vascular cryptogams had an upright axis possessing apical growth and radial symmetry, and producing lateral appendages in acropetal succession. These appendages were from the first of two kinds, viz. bracts or leaves and spore-producing members, and were closely associated together. The spore-producing members include sporangia, sori, and sporangiophores.

It will not be possible to give any full discussion of the conclusions here ; but a few remarks on one or two points may be made.

With regard to the view that the uncertainty of sexual reproduction on land puts a premium on an alternative mode of reproduction suited to dry conditions, it may be suggested that a biological explanation ought perhaps to give the prior place to dispersal. The dissemination of zygotes by water-currents having been lost, a means of wide dispersal becomes important, and multiplication without dispersal would not effectively meet the case.

The sporophyte is regarded as having been originally dependent on the parent prothallus (p. 218). The opposite view that the sporophyte generation may in the first instance have been independent of the gametophyte for its nutrition is perhaps dismissed in rather too summary a fashion (p. 226). The first is probably the natural view to any one committed to antithetic alternation ; but it may be pointed out that, if the hypothesis of homologous alternation be preferred, the original

independence of the sporophyte may be supposed to be possible, or even likely, considering that in so many fresh-water Algæ¹ the zygote undergoes a resting stage, during which the parent plant may die away.

Sterilisation and the theory of the strobilus, which figure so largely in this book, can only be referred to in a general way. Owing to altering conditions of competition and other factors, it is probable that fluctuating changes in size in the sporophyte and its appendages, and in the amount of branching, have always been very generally in progress. In view of such changes, combined with variable requirements of dispersal, it is to be expected that encroachment of the sterile on the fertile region (in the sporophyte or in the plant as a whole), and the converse, may have alternated many times in any direct line of descent. Consequently, evidence relating to these latter phenomena derived from living plants cannot have any great value in an attempt to establish the importance of sterilisation in the far-away ancestors of the Pteridophyta, or in forming a picture of primitive types.

Different views may be held on many of the theoretical points dealt with, as is fully admitted by the author; but that is a thing apart from the value of the book, which reaches a high standard of excellence in the nature and amount of the information and illustrations which it contains, and in the thorough and scholarly discussion of the theoretical problems concerned. The author expresses his belief that the full statement of even a speculative view will stimulate inquiry. Apart from the future influence of the book in this direction—and this will without doubt be far-reaching—the work has its own intrinsic value, which needs no apologetics, while the book itself together with the *Studies* (referred to above) form a good example of the stimulative value of a theory.

L. A. BOODLE.

Studies in Fossil Botany. Second Edition. Vol. i., Pteridophyta. By DUKIN-FIELD HENRY SCOTT, M.A., LL.D., Ph.D., F.R.S. [Pp. xx + 363.] With 128 illustrations. (London: Adam & Charles Black, 1908; price 6s. net.)

THE appearance of the first volume of the second edition of Dr. Scott's well-known *Studies in Fossil Botany* is a matter for congratulation not only to the author, but to all interested in the progress of palæobotany and botany. From the author's point of view, a demand for a further edition after the comparatively short period of seven and a half years must indeed be gratifying. To the public concerned, the revision and extension of the standard work on the subject, and one which more than any other has helped to spread and encourage a knowledge of fossil plants, is equally satisfactory.

The new *Studies* differs much from our former "guide, philosopher, and friend" the first edition. When the second volume appears it will be possible to arrive at some measure of the remarkable progress won during the past eight years by a comparison of the two editions. To bring up to date the ground covered by the *Studies*, as has been done in the second edition, has naturally necessitated expansion. We shall thus not be surprised to find that the new *Studies* is to appear in two volumes, though we gather that those who do not object to handling a somewhat bulky tome, as this new edition promises to be when completed, will be still able to obtain the work within the limits of a single cover.

¹ It is more probable that the land flora originated from fresh-water forms than direct from marine organisms.

Comparing the first volume, with which we are here concerned, with the single volume of the first edition, we find that its 363 pages and 128 illustrations contrast favourably with the 533 pages and 151 illustrations of the latter. The rise in price (an increase of three shillings where the two volumes are bound together, or of three and six when they are purchased separately), is happily not in proportion, and no one is likely to grudge the extra cost.

Yet while there is much that is new to the old edition, the scope and purpose of the work remain unchanged. The author's object is still to present "those results of palæontological inquiry which appear to be of fundamental importance from the botanist's point of view." His attitude on the whole has been very conservative as to the treatment and arrangement, and the present volume is in the main the former edition revised and brought up to date, so far as its limits extend. These limits include the Pteridophyta, the Spermatophyta being reserved for the second volume.

Some chapters have naturally required more extension and revision than others, and this is particularly the case with those devoted to the Ferns. The whole subject of the palæozoic ferns has had to be considered afresh in the light of the discoveries of the last few years. The account given in the present volume is the most complete we possess, and contains a description of some new and interesting members of the group.

As to the general excellence of the text it is unnecessary to speak. A word of praise may, however, be added in regard to the illustrations, which with few exceptions are adequate, and in all cases clear.

We notice one inconsistency, for which the author is no doubt not responsible. The book is called Volume I, on the title-page and in the preface, whereas on the cover it is labelled Part I. Certain sections, such as the descriptions of *Bothrodendron* and *Archæocalamites*, do not appear to be treated so fully as the rest, and might with advantage have been rather more expanded. But the author, no doubt in view of the space available, has perhaps wisely reserved for consideration at greater length those subjects which he has deemed the more important.

The literature, especially the more recent memoirs, is quoted, though not perhaps quite so fully as one could wish. It would be of great service to students, and also render this department more complete, if at the end of the second volume a bibliography of all the more important papers on structure-material were included.

E. A. NEWELL ARBER.

Introduction to Metallography. By PAUL GOERENS, Dr. Ing. Translated by FRED. IBBOTSON, B.Sc. (London: Longmans, Green & Co.; price 7s. 6d. net.)

THIS book is particularly useful for those who either intend to study metallography or who are actually engaged in work of this nature but are not acquainted with the fundamental principles underlying the science. In order to understand the structure of metals revealed under the microscope, it is generally essential to have a thorough knowledge of the critical changes that occur during their solidification and subsequent heat treatment. In Dr. Goerens' book, this branch of physical metallurgy is treated in a particularly lucid and interesting manner, and the reader is in no way troubled with the irrelevant matter frequently found in works on this subject. Only those portions of physical metallurgy necessary for metallographical purposes are given which would enable a beginner to obtain a knowledge of the methods of investigating metals and alloys.

The work commences with an account of allotropy and polymorphism, and then the author proceeds to indicate the exact nature of the cooling curves of different bodies. The various methods of obtaining cooling curves and plotting the results are described. It is perhaps a pity that the ordinary autographic methods of obtaining cooling curves were not more fully given; however, the compensation arrangement of Roberts-Austen for the determination of small thermal changes in steel and other alloys is clearly indicated, and so is the simpler form of the apparatus by Heyn.

After a description of the methods employed for the determination of thermal changes in bodies during heating and cooling, the nature of a physical mixture is discussed, and the properties of aqueous solutions generally are dealt with. The work of Guthrie, in connection with his experiments on the phenomena exhibited by various solutions of ice and salt during solidification, is fully described. Unfortunately, however, a mistake occurs in the freezing-point curve given in Fig. 24, where the separations of excess ice and salt respectively, in the under- and super-saturated solutions, are represented as horizontal straight lines, instead of convex ones, thus indicating that the whole of the ice or salt separates from the solution at a definite temperature. These separations, of course, really take place throughout the whole range of cooling, from the initial solidification to the eutectic line.

The solubility curve of ferric chloride in water by Bakhuis Roozeboom is fully described, and so are the various types of freezing.

After dealing with aqueous solutions and fixed salts, the author proceeds to show the analogies that exist between them and alloys, and he also incidentally illustrates the application of the phase rule to the study of metallic solutions. He divides the large number of alloys into groups according to the conditions determining their existence in the solid condition. Naturally binary and ternary alloys alone are considered, as in only a comparatively few instances have alloys containing more than three constituents been thoroughly investigated, since such researches are necessarily extremely complicated. The binary alloys are classified by the author in the following manner :

(a) No chemical compounds of the two components exist.

1. The two components form a continuous series of solid solutions.
2. The two components form a mechanical mixture after solidification.
3. The two components are partly soluble in each other; the alloy after solidification consists of solid solutions or mechanical mixtures of solid solutions.

(b) The two components form one or several compounds with each other.

1. The compounds are wholly or partly soluble in the components, or in one another.
2. The compounds are completely insoluble, reciprocally, as well as in the components.

The various types of binary alloys are illustrated by their equilibrium curves, and especial mention is made of pairs of alloys used extensively on a commercial scale.

The section dealing with binary alloys is treated in an eminently practical manner, and the essential properties of bearing metals, generally ternary alloys, are described. It is pointed out that a bearing metal should consist of hard grains, surrounded by a soft plastic ground-mass, so that should the bearing pressure become too great, the grains will be pressed into the plastic matrix, and the form of the bush adapts itself to that of the journal.

The chapter on practical microscopy of metals contains all the necessary information a beginner would require in order to polish samples of metal and develop their structure. It also includes much useful information in connection with the apparatus and the various methods employed for obtaining records of the results photographically.

The section dealing with the special metallography of iron and its alloys differs from the original German, as it has been re-written and brought thoroughly up to date by the translator. This portion of the work deals with the equilibrium curve and the microstructures of the iron carbon series of alloys in a very able manner. Not only are the "slowly cooled" structures illustrated, but the effects of varying heat treatment are also fully described.

Both the author and translator are to be congratulated on the production of this work, and we can most confidently recommend it to all who are interested in the science of metallography.

W. H. MERRETT.

Entomology, with special reference to its Biological and Economic aspects. By JUSTUS WATSON FOLSOM, Sc.D. (Harvard). (London: Rebman, 1906; price 14s.)

THIS work on Entomology is quite original in its treatment, and will be of great value when placed with such works as Berlese's *Gli Insetti*, and Sharpe's *Insects* in the Cambridge Natural History Series, for we shall then have a wide field of reference on this subject in its various branches.

The author divides the work up into thirteen chapters and an appendix dealing with Literature. The subjects of these chapters are as follows: Classification, Anatomy and Physiology, Development, Adaptations of Aquatic Insects, Colour and Coloration, Adaptive Coloration, Origin of Adaptations and of Species, Insects in relation to Plants, Insects in Relation to other Animals, Interrelations of Insects, Insect Behaviour, Distribution, and Insects in Relation to Man.

Many of these subjects will at once strike us as being of great interest. The chapter on Classification is brief, and might almost have been left out. The author treats insects under no less than seventeen different orders, which is to be regretted. There is no valid reason for placing the Fleas in a separate order, Siphonaptera; they are Diptera. It is surely unnecessary to place the neuroporous Panorpidæ as a distinct order, Mecoptera. The Termites he places in another order, Platyptera, and the Aptera are divided into two orders, Thysanura and Collembola. The chapters on Anatomy and Physiology are well worked out, and are interestingly written.

The subject of Colour and Coloration is treated very fully; the diffraction of light by fine close parallel striæ is shown to produce the brilliant iridescent hues of many butterfly scales. A species of the gorgeous genus *Morphio* is stated to have no less than 1,400 striæ on its iridescent pigmented scales per millimetre, and further the author explains that the interference colours of butterfly scales may be due also to the lamination of the scale and to the overlapping of two or more scales. Pigmental colours and their nature are also entered into. Seasonal and sexual coloration are dealt with, but not to the extent that they might have been. Much interesting reading is found in the chapter dealing with Insects in Relation to Plants. Notable amongst the information given here is that upon the "ant-plants." Belt found in Nicaragua species of *Acacia* with large hollow stipular thorns inhabited by ants of the genus

Pseudomyrma, the ants entering by boring a hole near the apex of a thorn. The plant affords the ants food and shelter. The ants obtain their food from glands which secrete a sugary fluid at the base of the petioles, and in return they protect the trees by driving away the leaf-cutting species. Dealing with Insects in Relation to other Animals, we do not find all we might expect, but there is much of interest, especially in the section on insects in relation to fishes and birds, in connection with food. The subject of fish food is very important to those who are in charge of "hatcheries," and in this respect we have much to learn from the researches in America. The author quotes largely from the valuable writings of Forbes in connection with this subject.

Some notes on the Transmission of Diseases by Insects are of course included, and the malarial parasite seems to come in for more detail than the mosquitoes, which form the intermediary host. We are not sure that the recommendations, however, given by the author in regard to preventive measures against malaria are quite up to the times. Any one living in regions infested by these insects will soon discard such advice as using washes of eucalyptus oil, oil of tar, etc., for none have any lasting effect in preventing their biting.

Under "Tropisms, Instinct and Intelligence," we find twenty-one pages of very thoughtful reading. The author explains very clearly the phenomena attending the environmental influences of light, temperature and moisture directing and controlling the locomotion by determining the orientation of the body—the *positive phototropic* moth flying to the flame, the *negative phototropic* cockroach avoiding the light, just as the *positively heliotropic* sunflower turns to the sun.

We are glad to see the following sentence in this chapter: "Though manifestly dominant, pure instinct fails to account for all insect behaviour. The ability of an insect to profit by experience indicates some degree of intelligence." Any one who has watched ants performing their work must surely know that intelligence, not mere instinct, guides them in their multifarious ways. That they show abstract reasoning is another matter.

Insects in Relation to Man is the only disappointing chapter; it could not even touch the fringe of this vast subject in sixteen pages, but what little it does touch is accurate.

The work, although published in this country, is essentially American, but we are pleased to see it is written in the mother tongue, and that the strange spelling—certainly not English—is conspicuous by its absence. The numerous illustrations are excellent, and the whole work is full of thoughtful reading for students of nature in all parts of the world. One is refreshed to find oneself in fresh fields and pastures new, and one can say this of this book, which can only help to make the subject of a more interesting and useful nature.

FRED. V. THEOBALD.

A Note on the Lac Insect (*Tachardia lacca*): Its Life-History, Propagation, and Collection. By E. P. STEBBING, F.L.S., F.Z.S., F.E.S., Imperial Forest Zoologist. *Indian Forest Records*, vol. i. Pt. 1. Price Rs. 1. 4. 0. (1908).

A REPORT of great interest has just been brought out by Mr. Stebbing on the lac insect. That lac is the product of an insect—one of the Coccidæ—is well known. Its great importance to India can be estimated by the custom returns, which show that the value of the export from the maritime ports of India for 1905-6 amounted to no less than 3,31,39,786, or nearly 3½ crores of rupees! In investigating this

subject Mr. Stebbing is doing not only work of interest but of great economic importance.

The report gives the first good account of the coccid in all its stages and an account of its life-history. Then follows a list of lac-yielding trees—acacias, albizzias, atrocarpus, anona, etc., as well as cajanus, butea, ficus, and zizyphus—from which commercial lacs are obtained, and the *Schleichera trijuga*, which yields the finest lac of all and is the most important of all lac-bearing trees.

Speaking of the injury done to the trees by the lac insect, Mr. Stebbing says: "It is probably but rarely that the trees are ever killed by the insect."

This valuable scale-insect is preyed upon by various enemies, such as the larvæ of *Galleria* and *Eublemma*, but we learn that very little is known about them.

Valuable chapters follow on its distribution, exploitation, and the necessary steps to be taken to improve its cultivation and collection. The whole must prove of the greatest service in stimulating and improving one of the many resources of India. If the beautiful cochineal dyes are or have largely been superseded by the fugitive and crude chemical colours, this insect product is more than holding its own.

FRED. V. THEOBALD.

On Some Assam Sal (*Shorea robusta*) Insect Pests. By E. P. STEBBING, F.L.S., etc. *Forest Bulletin* No. 11 (1907). Price 2s. 6d.

THIS is the first account of the insect enemies of this valuable Indian tree, which is too frequently defoliated by the caterpillars of various moths, as *Dasychira* and *Lymantria*.

Amongst the various enemies dealt with is the Sal-borer (*Hoplocrambus spinicornis*, Newn.); *Dasychira horsfieldi*, Saund.; *Lymantria mathura*, Moore; and new species of *Sphaerotrypes*, *Thanasimus*, and several new *Scolytide*. Life-histories and preventive and remedial measures are given, which, with the excellent plates, form a work of not only scientific but great practical value.

FRED. V. THEOBALD.

Text-books of Physical Chemistry: Thermochemistry. By JULIUS THOMSEN, Emeritus Professor of Chemistry in the University of Copenhagen. Translated by Katherine A. Burke. [Pp. xv. + 495.] (London: Longmans, Green & Co., 1908; price 9s.)

EXCEPT for a short account of experimental calorimetric methods taken from the *Thermochemische Untersuchungen*, this volume is a translation of Prof. Thomsen's more recent Danish work, *Thermokemiske Resultater*. In this latter book he has summarised the results of his laborious thermochemical investigations, which are presented here for the first time in a readily accessible form. To the extraordinary energy and zeal of the investigator this valuable compendium of thermochemical data bears eloquent testimony; it is an instructive example of what may be achieved by a single scientific worker who devotes his life to research on one particular line.

While welcoming another of these Text-books of Physical Chemistry, one may at the same time express regret that it has not been possible to provide a volume on Thermochemistry written especially for this series, and dealing with this branch of the science from a detached point of view. An account, such as is given in this book, of the researches of an investigator like Thomsen is bound to

be both interesting and stimulating, but it can scarcely be claimed that a student would obtain from a perusal of the volume an all-round and up-to-date acquaintance with thermochemistry. The book, being simply a record of the author's original investigations and conclusions, contains almost no reference to the work of others; it is noticeable that, while there is an exhaustive subject index, there is no name index. So far as can be seen from a cursory inspection, there is not even a reference to Berthelot's method of determining heats of combustion, and it must be admitted that such limitations of the scope of the book, however excellent from the author's point of view, somewhat impair its usefulness as a general text-book.

Again, owing partly to the fact that Prof. Thomsen's collected researches were published by 1886, before the electrolytic dissociation theory was brought forward, partly, perhaps, to the author's own views on the subject, the application of that theory in interpreting the thermochemical behaviour of electrolytes is not recognised in this volume. The translator, whose work is generally satisfactory, tries to make amends by one or two verbal alterations, but this of course is only a partial remedy, and the reader is asked in the translator's preface "to place the modern interpretation on the figures given should he so desire." Surely this leaves the student rather in the lurch!

The bearing of the electrolytic dissociation theory on the thermochemical behaviour of electrolytes is very direct, and receives perhaps its most important illustration in the fact, elucidated from Thomsen's figures and recently emphasised, that the heats of formation of the salts in dilute solution are additive quantities. This is a notable result, especially when it is borne in mind that no such relationship exists in reference to the heats of formation of the solid salts or their heats of solution. For a discussion, however, of these and allied thermochemical results in the light of modern theories, one searches Prof. Thomsen's book in vain.

The expectations that were once entertained as to the importance of thermochemical investigations have not been realised. Specially is this the case in reference to the principle of maximum work—generally associated with the name of Berthelot, although Thomsen himself in the volume under review claims to have propounded it thirteen years before Berthelot. In any case, the principle of maximum work has lost its significance, and it is now recognised that the quantity of heat evolved in a chemical reaction is not an accurate measure of the affinities of the reacting substances. The value of thermochemical data in deciding questions of constitution has likewise been exaggerated, and to-day there are other physico-chemical methods which are much more serviceable in this respect.

But even when due allowance is made for the fact that thermochemical research has not yielded results of such significance as was at one time anticipated, there is no doubt that the study of the heat effects which accompany chemical changes will always be an important branch of investigation. At the present juncture, when Berthelot is dead, and Thomsen has retired from active work, one wonders who the thermochemists of the immediate future are to be whose work will rank with that of these two masters.

J. C. PHILIP.

Thermodynamics of Technical Gas Reactions. By FRITZ HABER. Translated by Arthur B. Lamb. [Pp. xix + 356.] (London: Longmans, Green & Co., 1908; price 10s. 6d.).

PHYSICAL CHEMISTRY, on its theoretical side, is largely based on thermodynamics, and the present volume furnishes a fresh example of this close relationship. More than that, it represents an endeavour to bring thermodynamics into touch with

technology, and in this respect is a manifestation of the present tendency to bring the theorems of physical chemistry to bear on the problems of technical chemistry. An example of successful work on these lines has been already furnished by the application of the phase rule to such problems as the nature of iron-carbon alloys. In this and other cases it has once again been found that what appears to be nothing more than "pure" science may turn out to be most necessary to the sound advance of "applied" science. The two things are complementary to each other, not mutually exclusive.

The volume under review had its origin in a course of lectures delivered some three years ago by Prof. Haber to an audience fairly familiar with the chemical and technical side of the subject. A good deal of space is therefore devoted to the development of the mechanical theory of heat, and the author's treatment of this subject will be found fresh and illuminating by those readers who are chemists rather than mathematicians. The author has chosen Helmholtz's point of view, and regards a chemical reaction as having a latent heat just like any simple change in the state of aggregation. It is claimed that the theoretical and practical connection between gas reactions and the dissociation and vaporisation of solids can be much more readily appreciated from this standpoint.

Owing doubtless to the author's anxiety to provide a sure foundation, the first hundred pages or so of the book contain very little that has anything directly to do with technical gas reactions, so that the chemical reader especially must be prepared to exercise patience. Among the gas reactions which are discussed from the thermodynamical point of view are the union of nitrogen and oxygen in the electric arc, the Deacon chlorine process, the water-gas reaction, and the contact process for the manufacture of sulphuric acid. Special emphasis is laid on the importance of specific heats in gaseous reactions, and an interesting chapter is included dealing in detail with the methods used and the results obtained in this field. In view of the amount of work which is being done at present on the problems discussed in this volume, it is satisfactory to find that appendices to several chapters have been specially written by the author for the English translation, so that the latter has been brought up to date.

The translator has done his work satisfactorily, and there is little trace of the outline of the original German sentences, such as one can frequently detect in translations of this kind. Occasionally a little more independence of the original might have been shown, as for example, in the choice of a symbol for "room temperature"; the letters *Z. T.* betray their origin. This, however, is a trivial matter, scarcely worth the comment, when the translator's work is so generally excellent.

J. C. PHILIP.

Modern Electrical Theory. By NORMAN R. CAMPBELL. [Pp. xii. + 332.] (Cambridge: University Press; price 7s. 6d. net).

NOT the least characteristic feature of this book is the tendency of the author throughout to act as his own critic. This peculiarity appears even in the prefatory description of the scope of the work. While stating that his original intention was to provide a book which would appeal to students already well grounded in physics but unacquainted with modern developments, the author admits that a considerable part of the work will be already familiar to such students. Similarly in the book itself, from time to time, some particular point of view is presented, and then, as likely as not, a statement follows of effect equivalent to this: "It seems

that the phenomena are by no means so simple as has been represented above, and it is likely that many of the statements made will be proved to be untrue" (p. 196).

Again, after a spirited attack upon a particular conception of the aether and certain victims of the "crudest nominalism," for whom, after all, there is more to be said, the author admits that (what appears to be) the fundamental conception of his book may prove "as misleading as that of the aether" (p. 317).

No exception need be taken, within limits, to the author's standpoint, that any conception is worth considering which has the merit of raising questions that some older conception conceals. But many conceptions, at any rate, are mere temporary expedients in the attempts at correlation of phenomena, and there is the danger that excessive multiplication of conceptions, or of modifications of a particular conception, in a book aiming at giving a general account of modern electrical theory, will serve only to bewilder the general scientific reader.

The author would probably admit that the book will appeal far more to the expert than to any one else. Indeed, when the fundamental concept of modern theory as here presented—the electron and its associated "Faraday" tube—is still so elastically specified that from each electron there may proceed one tube, or many, to infinity, or to infinity and back again (pp. 316, 320), it seems clear that the time is not yet opportune to present the attempted correlation of phenomena to such a person, if there is one, as a non-electronic physicist. For the book as a means of stimulating thought and investigation by experts, however, there can be nothing but praise.

Some further remarks are suggested by perusal of the book.

After the author's plea for exactness of expression, as on p. 313 and elsewhere, one is tempted to ask how two or more tubes of like direction can pass through the same point (Prop II. p. 10).

The statement in the middle paragraph of p. 251—"the conclusion . . . is so attractive that it seems desirable to accept it provisionally"—is not a very convincing mode of expression.

Besides a few obvious misprints, we have noticed that on p. 25 the mass per unit length is accidentally written v^2 less than the actual value. In the interesting method of exhibiting the velocity of propagation of electric effects along tubes given on pp. 30, 31, the expression for V from which equation (1) is derived should either contain N^2 instead of N under the square root in both numerator and denominator, or a necessary modification should be made in the text.

Even if the remark were true, it seems unnecessary that the author should point out (p. 254) that the "ordinary theory of chemical affinity" merely restates the facts in such a way as to obscure their significance. It yet remains to be seen whether the electronic or any other attempted restatement of facts may not suffer, at least a little, from the same objection. In any case, the author's own remark at the top of p. 255 in connection with chemical affinity, is either incomplete or thermo-dynamically incorrect.

It does not appear from the context why the author introduces the very cumbrous method of accounting for Airy's experiment on the aberration of light given on pp. 293, 294, and he makes the erroneous statement on p. 289 that the ship and shot analogy in connection with aberration is due to Wood. It was used by Airy sixty years ago or more.

The enthusiasm of the author for his subject leads him to make more than one extraordinary statement. Not the least extraordinary is: "My confidence in the genius of Faraday is so great that I should be inclined to accept any hypothesis,

however rash, which was suggested by the ideas that he developed" (p. 322). One wonders whether a resuscitated Faraday would be prepared to go as far.

A remarkable feature of the book is the absence of differential equations, and of higher analysis generally; but whether the author is right or wrong when he says "a page of symbols covers a multitude of assumptions," the same is certainly true of some pages of his own prose.

There are many who would hold, in the light of the experience of the past, that the author (on p. 286) asks more reverence for the electronic theory than it can reasonably claim. The electronic is not the first theory which has proposed, amongst other things, to "bridge the gulf between chemistry and physics" (which, in a sense, never existed), and there is some reason to suppose it will not be the last.

Criticisms of this kind, however, refer only to minor characteristics of the book. As a whole it is a notable and stimulating piece of work, for which the author deserves the gratitude of all who have the desire to assist in the promotion of knowledge at heart.

An excellent feature of the book must be mentioned in conclusion. At the end of each chapter a summary and references to the chief papers on the subjects discussed are given, as well as brief indications of the contents of some of the latter. This is an invaluable service to the reader.

S. W. J. SMITH.

The Chemical Basis of Pharmacology. By FRANCIS FRANCIS, D.Sc., Ph.D., and J. M. FORTESCUE-BRICKDALE, M.A., M.D. [Pp. xii. + 372.] (London: Arnold, 1908; price 14s. net.)

IN this work, which is one of the most comprehensive treatises of its kind in the English language, the authors supply a chemical basis for the pharmacology of the organic substances now employed as therapeutic agents in general medical practice. The aim in view is indicated in the preface, where the hope is expressed that the book will enable medical students to realise more fully the connection between organic chemistry and the pharmacology of the present day, and also that it may tend to broaden the teaching of *materia medica*, so as to include a consideration of the numerous synthetical remedies which are now so prominently brought under the notice of the modern physician. From this point of view, the book may be regarded as a chemical "Who's Who" for drugs which are almost invariably advertised under trade names intended frequently to mask, rather than to reveal, the true nature of the substances in question. The index, which contains the names of the more important compounds in heavier type, facilitates the use of the work for purposes of reference. A few not unimportant drugs have, however, been omitted. For example, the well-known diuretic, piperazine, is neither indexed as such nor indicated by its more scientific name of diethylene-diamine.

The extent to which this treatise will enable medical practitioners to gauge the merits or demerits of the host of synthetical remedies now thrust upon them can best be appreciated after reading what are perhaps the most suggestive sections of the work—namely, those dealing with the rational and empirical methods of therapeutics and the difficulties attending the attempt to correlate chemical constitution and physiological properties. The authors adduce many cogent examples showing that the physiological action of a drug is influenced by its physical as well as by its chemical properties, that little is known as to the way

in which the cell protoplasm reacts with drugs, and that the selective interaction of cells and drugs stands in want of a chemical explanation.

A few generalisations are apparent, such as the general curarine-like action of quaternary ammonium bases and the depression in physiological reactivity induced by carboxyl and sulphonic groups; but these and other similar rules are subject to variation and exception. The abandonment of empiricism is as yet outside the range of the practical study of medicine, and accordingly pharmacological research must for the time being proceed very largely on empirical lines. This is a matter deserving of very earnest consideration in this country, where an almost impassable gulf exists between the chemist who synthesises new types of highly reactive substances and the pharmacologist who could demonstrate the utility or otherwise of these products by the only available test of experiments carried out *in vivo*.

Although a rational explanation of the action of drugs based on chemical constitution is not possible on account of our present ignorance of the chemistry and reactivity of the living cells, yet within certain narrow limits a correlation may be traced between definite chemical variations and the ensuing pharmacological results. The authors call attention to many such relationships, and an interesting case is summarised on pp. 207-12, where the constitution and physiological action of the aromatic antipyretics are discussed. Of special interest in this connection are the sections relating to the theories of hypnosis, the bactericidal values of the aromatic antiseptics, the salol principle, the alkaloids and their synthetical substitutes, and the dependence of taste and odour on chemical constitution.

The rapid advance in the production of new remedies has already placed on the market several important drugs, such as atoxyl and synthetical suprarenine, which have come into vogue since this book was written, but in a future edition it might be possible to incorporate these and other new developments, without enlarging the work, by deleting the more elementary portions of the organic chemistry, which could be conveniently studied in the existing treatises on this subject.

G. T. MORGAN.

A Manual and Dictionary of the Flowering Plants and Ferns. Third Edition. By J. C. WILLIS, M.A., Sc.D. [Pp. xii. and 712.] (Cambridge: University Press, 1908; price 10s. 6d.)

A MANUAL and dictionary on whatever subject is a work which it is only fair to both author and reviewer to criticise after having had an opportunity of putting the book to the test of actual use during a considerable period of time. My own copy of the first edition of Willis's dictionary first came into my hands in Ceylon in 1897. Since then it has been in constant use in England and the West Indies, and formed part of the very scanty library which alone was practicable during a tour in West Africa. It is during such time, when very few books are available, that one realises what a wealth of readily accessible information there is within the covers of this unpretentious-looking volume. The appearance of the third edition within such a comparatively short period as four years since the publication of the second is sufficient evidence that our own opinion of the value of the book is shared by many others.

In the event of there being any readers of this notice to whom the general plan of the book is not familiar, it will be sufficient to note that it is divided into three parts, preceded by an introductory chapter, which deals, amongst other useful

subjects, with the object and use of botanic gardens and museums, and gives sound practical hints to field botanists and collectors.

Part I. (pp. 19-207) is practically a botanical text-book, in which excellent features are the treatment accorded to the biology of the flower, and to the forms of vegetation and the geographical distribution of plants.

Part II. (pp. 211-643), the main portion of the book, contains an account of the classes, cohorts, orders, and chief genera of plants arranged alphabetically under their scientific names. For reference purposes this part is invaluable, and with the improvements effected since the first edition the inquirer will usually find the order and geographical distribution of a plant; those of biological or economic interest are well described, whilst large groups and important or interesting plants are very fully treated, *e.g.* the account of *Filicineæ* occupies five pages.

Part III. consists of a comprehensive glossarial index.

To visitors to botanical gardens who wish to learn something of plants bearing only their scientific names, travellers in foreign lands, in addition to the professed botanical student at home, this book can confidently be recommended as one of the most useful of modern botanical works of reference.

W. G. FREEMAN.

A List of Official Chemical Appointments. Second Edition. [Pp.172.] Compiled by RICHARD B. PILCHER, Registrar and Secretary of the Institute of Chemistry. (The Institute of Chemistry, London, 1908; price 2s.)

THIS list has been compiled, under the direction of the Council of the Institute of Chemistry, with a view to affording information to students intending to take up chemistry or any of its applications as a profession. In addition to giving a list of official appointments open to chemists, and of the names of their present occupants, it specifies, where possible, the conditions under which these posts are held, and the qualifications insisted upon by the authorities in whose hands appointments to these posts are vested.

The contents are arranged alphabetically, and there is an excellent index of names of persons holding official appointments, so that the information given on any point can be referred to readily. Nothing but actual everyday use can satisfactorily test the utility and trustworthiness of a compilation of this description. This test the first edition answered well, and the second edition, which is somewhat wider in scope, seems to have been prepared with equal care.

T. A. II.

HEREDITY AND RADIUM AT DUBLIN

AN IMPRESSION OF THE BRITISH ASSOCIATION MEETING

BY W. BEACH THOMAS, M.A.

THE stimulating air of Dublin had a very visible effect on the British Association. The members were in great force, and among Associates were fewer of those very unattached camp-followers who enjoy not Science but the reputation which is bought cheaply at a guinea. The name of Darwin as well as the air of Dublin no doubt added impetus to the meeting. People wanted to hear about Darwin, and one of the mistakes of the week was that they were allowed to hear so little. No speech was more popular than the few words, very beautifully said and delivered, in which Sir Archibald Geikie explained how in his later years Charles Darwin found an encyclopædia in his sons, getting mathematical and astronomical facts from George, botanical facts from Francis, financial and mechanical help from the others.

It seemed to many a mistake that the President, Mr. Francis Darwin, who has written delightfully on his father's life, held aloof from evolution. A big popular audience was longing to hear what this satisfying word, which can be safely used in the drawing-room, really meant. The President is a good Darwinian, a firm believer in the adequacy of natural selection, recently much out of favour with men of Science. Instead he plunged almost without an introduction into very difficult questions of cell development. The gist of the lecture was as severely handled by botanical critics as by the popular audience. Yet the contribution was original, and the short summing-up of his last paragraphs as plain and direct as, may one say, the concluding sentences of those inexplicable rigmaroles which so often made an hour's prelude to the decisive climax of Cromwell's speeches.

It seems to me certain (said the President), that in development we have an actual instance of habit. If this is so, somatic

inheritance must be a *vera causa*. Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings exhibit in the plainest way the universal presence of a mnemonic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.

This is at any rate a plain challenge to Weissmann, and what follows is an equally plain defence of natural selection.

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemonic hypothesis of evolution makes a positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that cannot learn is an integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break.

As I listened to the slow and unemotional delivery of this interesting and unorthodox address, it occurred to me how differently it would have impressed the audience if they could have been reminded of a delightful passage in Charles Darwin's autobiography, edited by the speaker. The simplicity of it makes it one of the great documents of the world, and this said address appears in the light of it as an incomparably Darwinian product. The passage is this :

In 1880 I published, with Frank's assistance, our *Power of Movement in Plants*. This was a tough bit of work. The book bears much the same relation to my little book on climbing plants which *Cross Fertilisation* did to the *Fertilisation of Orchids*; for in accordance with the principle of evolution it was impossible to account for climbing plants having been developed in so many widely different groups unless all kinds of plants possess some power of movement of an analogous kind. This I proved to be the case; and I was further led to a rather wide generalisation—namely, that the great and important classes of movements, excited by light, the attraction of gravity, etc., are all modified forms of the fundamental movement of circumnuta-

tion. It has always pleased me to exalt plants in the scale of organised beings; and I therefore felt a special pleasure in showing how many and what admirably well-adapted movements the tip of a root possesses.

That one sentence, "it has always pleased me to exalt plants in the scale of organised beings," might have stood as the motto both of the President's speech and of Dr. Haldane's to the Physiological Section. Many of the chemists called Dr. Haldane's speech retrogressive. He went near to revive the old "vital principle," which did duty in old times for rational explanation, and delivered some undiluted Bishop Berkeley. Sir William Crooks could perhaps find in the fact an indication of the surviving influence of Berkeley's spirit. Was he not in his day a fellow of Trinity College, Dublin? But the address, which had some curious analogies with the President's, was an overwhelming criticism of the physico-chemical view of life. The argument, as most arguments this year, sprang from a consideration of heredity.

I confess (said Dr. Haldane) that as a physiologist I am struck with amazement at the manner in which heredity is often discussed by contemporary writers who endeavour to treat the subject from a mechanistic standpoint. Sometimes, indeed, the germ-cell is acknowledged to be a complicated structure, but at other times it is treated as a "plasma," which can be mixed with other "plasma," divided, or added to, as if for all the world it were so much treacle! I have tried to place clearly before you the assumptions in connection with heredity which to my mind make the physico-chemical theory of life unthinkable, even if it be tenaciously clung to in connection with those ordinary physiological phenomena where it has proved so disappointing.

The conception which he substitutes for the more material, even in its most ingenious form of expression, is very near "vital force," though it may be none the worse on that account.

The conception which is to take its place is simply the conception of the living organism, which stands; or ought to stand, in the same relation to Biology as the conceptions of matter and energy to Physics, or of the atom to Chemistry. A living organism is distinguished by the fact that in it what we recognise as specific structure is inseparably associated with what we recognise as specific activity. Its activity expresses itself in the development and maintenance of its

structure, which is nothing but the expression of this activity. Its identity as an organism is not physical identity, since from the physical standpoint the material and energy passing through it may be rapidly changing. In recognising it as an organism we are applying an elementary conception which goes deeper than the conceptions of matter and energy, since the apparent matter and energy contained in, or passing through, or reacting with, the organism are treated as only the sensuous expression of its existence. Even the environment is regarded as in organic relation with the organism, and not as a mere physico-chemical environment. It follows that for Biology we must clearly and boldly claim a higher place than the purely physical sciences can claim in the hierarchy of the sciences—higher because Biology is dealing with a deeper aspect of reality. It must also be the aim of Biology gradually to penetrate behind the sensuous veil of matter and energy which at present seems to permeate the organic world at all points.

This conception leads him to a conclusion on heredity in which Mr. Francis Darwin's very word plant "memory" is used, though with apology.

If (he argued) it is a fundamental axiom that an organism actively asserts or maintains a specific structure and specific activities, it is clear that nutrition itself is only a constant process of reproduction: for the material of the organism is constantly changing. Not only is there constant molecular change, but the living cells are constantly being cast off and reproduced. It is only a step from this to the reproduction of lost parts which occurs so readily among lower organisms; and a not much greater step to the development of a complete organism from a single one of the constituent cells of an embryo in its early stages. In all these facts we have simply manifestations of the fundamental characters of the living organism. The reproduction of the parent organism from a single one of its constituent cells separated from the body seems to me only another such manifestation. Heredity, or, as it is sometimes metaphorically expressed, organic memory, is for Biology an axiom and not a problem. The problem is why death occurs, what it really is, and why only certain parts of the body are capable of reproducing the whole.

The question of heredity held the field, and beyond all doubt the most striking fact about the meeting was the universal interest in the year's sum of work on the law of Mendel. The Abbot of Brunn was as much the hero of the occasion as Darwin; and it is curious to remember that fifty years ago the chief reason why the law was forgotten at its first publication

was the overwhelming interest aroused by *The Origin of Species*. At the jubilee of that work the positions are reversed. This strange exception to the Darwinian laws is filling the minds of men of science the world over. "Dominant" and "recessive" are words rapidly leaking out into popular speech, and will soon be rivals to the blessed word "evolution." There is of course, too, a conscious rivalry. The President's address, in some respects ultra-Darwinian, was perhaps more severely dealt with in private discussion than any speech of recent years. The inheritance of acquired characters is, as he said, for the moment a lost cause. Many felt this; but only the Mendelians came into the open on the subject. Professor Bateson interrupted a lucid exegesis of the law to beg his audience not to follow the vague philosophies of the President, but to believe that the only path of real advance was by close and faithful observation of such facts as were occupying Mendelians in all parts of the world. He waved his triple branch of sweet peas as a fragrant wand of protection against all who were not within the Mendelian pale, and not even Professor Poulton, who was the most potent enemy present, took up the challenge. The three sweet peas are, if one may say so, the trump card—better winners of tricks even than the blue Andalusian fowls, whose blue blood must surrender to the plebeian stock from which it sprang. From the Invincible sweet pea spring, do what you will, Painted Lady and Duke of Westminster; and what in the world becomes of cell-memory before the due proportional recurrence of these different but persistent forms?

Professor Bateson's speech was as eagerly expected as any at the meeting. Since the Association Meeting of 1904 he has been our Mendelian protagonist. We were also expecting momentous announcements of the results of the year's work. And from the Board of Agriculture itself had issued rumours of saccharine turnips and heavy-wooled sheep, which were about to prove to the practical world of farmers that Science was their friend. But Professor Bateson, partly because he had lost his slides, partly because he thought that his duty was to talk down to the young intelligence of the youngest of the sub-sections, did no more than give an elementary sketch of the meaning of the law. The whole of the ground of his lecture was covered at Leicester last year, and very clearly given by Mr.

Punnett. Even the slides were not new, and that remarkable but very well-known pedigree of night-blindness, completed years ago by Professor Nettleship, again did duty. Of course the British Association must be to a degree popular. It wants to advance the public interest in science; but here again I would insist on the fact that the public is far more generally interested in new facts than in didactics or explanations of old facts. The Association would be reduced to fatuity if all the new sections take the view that their duty is not to communicate the year's progress, but to explain the rudiments. A phonograph record taken last year would have served the purpose of the Mendelian debate admirably. Happily one new fact leaked out, as if against Professor Bateson's will. He seems to have proved almost to the hilt that the peculiar "pacing" action of the American horse responds as clearly as the colour of the hackney to the Mendelian law. One very suggestive contribution was made to this debate by Professor Wilson, who gave some admirable illustrations of the way in which biological laws may test historical views and suggest historical studies. The application is quite new. His sketch of the supposed migrations of varieties of cattle through England and Ireland was too long even to summarise in this place; but the conclusion is enough to exhibit the possibilities of his new historical method.

It was supposed, on a tradition largely oral, that the Shorthorn was created out of two distinct breeds, a red and a white. This historical conjecture is biologically proved by Mendel's law. Statistics from Shorthorn records indicate that roan Shorthorns produce red and white and roan young according to the Mendelian ratio: the roan Shorthorn, one may say, is an exact parallel with the blue Andalusian. The history of Dexters and Kerrys is similar; but their breeding produces some eccentricities that might well be investigated.

The Dexter is a cross between the black cattle of Kerry and the red cattle of the South of England. In crossing the black and red, black is dominant over red and the stout animal dominant over the tall. Dexters as a rule are not bred with Dexters. The safe way to get a Dexter is to breed from a Kerry and a Dexter. But when bred together the Dexters breed in addition to Kerrys and Dexters a large proportion of animals that are really monstrosities, with an unnatural

number of legs or tails. The fact has long troubled breeders, and not even local superstition has a theory.

Professor Wilson's argument, which a map marked with lines of migration made very persuasive, suggests that the sciences and arts might help each other much more than they do. How often the doctor would help the historian! What a number of critical events have depended on nothing more nor less than a state of health! A Prime Minister resigns because he cannot sleep, a Napoleon misses the critical point in a battle because the cancerous doom of his family begins to exert its influence. An admirable example briefly alluded to at one of the Dublin debates was provided by a paper published last year, in which it was argued that the decay of Greek and Roman civilisations was profoundly affected by the introduction of malaria. Historians are only just beginning to appreciate even archæology, the disregard of which ruined the value of Grote, and indeed most of the older historians, before the Evanses and Mr. Hogarth began to dig up lumps of concrete history that knocked down the most carefully erected theories. How much more accurate and valuable, and indeed readable, their history would be if they took evidence also from biology and physiology, or even physics! After all, was not Halley's Comet, about which Mr. Turner talked pleasantly at an evening lecture, directly concerned with the conquest of England in 1066? What war its arrival in 1910 will entail, Mr. Turner did not say.

Has Mendelism anything to say on the question of inbreeding? This question is suggested by the accidental fact that Professor Wilson passed direct from his history of cattle as tested by Mendelian inheritance to the pedigree of certain famous animals. Every one acquainted with breeding knows that our early herds were made and kept pure by very close inbreeding. Bakewell travelled all over England and perhaps the Netherlands to find an animal as good as his own. When he failed he inbred, on the theory that whatever happened you must breed from the very best: *fortes creantur fortibus et bonis*. But perhaps none of his audience had quite realised the extent of the inbreeding. The most famous of all Aberdeen Angus cattle—and it is pertinent to remember that the animal had stamina and won prizes at eleven years old—issued from such close inbreeding that the whole of his ancestry over seven generations, giving a possible total of 128 ancestors, numbered only five.

Much the same thing happened with the early prize-winning Shorthorns, and breeders are now again coming back to the view that mixture of blood is quite unnecessary. If they are right, if no one can discover a weakness—say a pronounced tendency to tuberculosis—in these close-bred beasts, the fact seems to destroy the Platonic analogy between the breeding of beasts and men. One wonders if Professor Ridgeway—whose Presidential speech might have been cut from the *Republic*—noticed this point. It would form a pretty comment on the vigorous passage in which he rated the State for being a bad stockmaster and refusing to breed wholly or principally from the middle class.

In our community the children of the working classes are educated at the cost of the State, the offspring of the wastrels are given free meals, and already there are demands that they shall be clothed at the expense of the ratepayers, and that the parents shall even be paid for providing them with lodging. It is not impossible that before long these demands will be conceded by either party in the State. The heavy additional expense incurred in this policy falls upon the middle-class ratepayers and taxpayers, who have to feed, educate, and clothe their own children at their own expense. It may be said that they can get free education for their children by sending them to the State schools; but this is to level down instead of to level up, for if they do so they will be lowering the general morale of their own class, the most priceless asset of the nation. The heavy burden of taxation entailed by this policy, falling as it does with special weight on the middle classes, renders it more difficult each year for the young men and the young women in that class to marry before thirty, for they naturally shrink from the expense of bringing up large or even moderate-sized families. We need not then wonder at the falling-off in the rate of increase of the middle classes. Our legislators are bad stockmasters, for they are selecting to continue the race the most unfit physically and morally, whilst they discourage more and more the increase of what we have proved to be the outcome of a long process of natural selection. The present policy therefore tends to reduce that which in all ages has been the mainstay of every State, the middle class.

The problem of heredity was discussed in one form or another in five of the sections, and chiefly in relation to Mendelism. One almost expected Professor Joly to insert F^2 after radium when he described it as the grandson of uranium. Many zoologists are among the converts, including Mr. Sidney

Harmer, President of the section. He put the law in the forefront of his address, and made some hopeful suggestions.

The present century (he said) has seen a remarkable development of the study of the problems of heredity and variation, largely as the result of the interest awakened in the resuscitation of Mendel's experimental work from the oblivion in which it had remained for so many years, though the general problem is being attacked concurrently by investigators who attach more importance to the statistical method of study. Professor Bateson has given the name "Genetics" to the experimental study of heredity. Some of the more recent conclusions of the workers in Genetics are to be discussed by this Section during the present meeting. It cannot be doubted that an accurate knowledge of the principles of heredity is destined to exert a marked influence on the practical concerns of humanity.

That was a sonorous generality appropriate to a Presidential address, but Mr. Harmer had in his mind a precise application of the law to his particular subject. He has been studying the protozoa, and been much struck with the many unsolved problems and difficulties connected with the variations of certain remarkable appendages known as "avicularia." He found what he calls vicarious or adventitious avicularia of a highly specialised description associated in one and the same family with the normal type of avicularium, and could not account for their presence, but made this suggestion—very remarkable as coming from a precise zoologist deeply absorbed in a particular inquiry :

It appears to me worth while to suggest that some of our difficulties might be removed by appealing to the results obtained by workers on Mendelian inheritance. An essential part of the theory here involved is that in the formation of the gametes of an organism there is a segregation of certain paired or "allelomorphic" characters whereby some of the gametes are endowed with qualities by virtue of which they transmit one of the characters, while the rest of the gametes become capable of transmitting the characters of the other member of the allelomorphic pair. It has recently been made probable by Professor Bateson, whose views have been confirmed by others, that the actual appearance of a particular character may be dependent on a coupling of two allelomorphs belonging to distinct pairs. If only one of them is present the character will not show itself. The phenomenon of reversion on crossing is thus explained as due to the combination of allelomorphs present in the isolated condition in two parental forms.

Is it not possible that the perplexing occurrence of vicarious avicularia in some of, but not by any means in all, the colonies of certain species may be interpreted as a reversion due to the combination of two or more allelomorphs that may not have occurred together in the parental forms?

The President's example was followed when the zoologists and botanists met to inquire into recent work on the determination of sex. By far the most original contribution to the debate was Mr. Doncaster's record of breeding from moths. He began by sketching the early history of the question of sex determination till it reached the stage where Wilson and others discovered that in certain insects the males and the females contain different numbers of chromosomes in the germ-cells, the females having an even number, the males one less. After maturation there are two kinds of spermatozoa, one containing the same number as the mature egg, and the other having one chromosome missing. It was at first suggested that at fertilisation the spermatozoon having the larger number caused the egg to develop into a female, that with the smaller number male; but Wilson's later suggestion is that there is selective fertilisation, that the eggs are either male or female, and that male eggs are fertilised by spermatozoa having no heterochromosome, female eggs by those which have it. Morgan has recently found that in a species of *Phylloxera* there are two kinds of spermatids, one of which has one chromosome more than the other. Those with the smaller number degenerate; those with the larger develop into functional spermatozoa, and all fertilised eggs become females.

One particular experiment illustrates this remarkably. In the moth *Abraxas Grossulariata* is a rare variety *lacticolor*, which is found usually only in the female. This acts as a Mendelian recessive, and the tables of generations were extremely remarkable. From them Mr. Doncaster argued that the sex determinants behave as Mendelian characters, maleness and femaleness being allelomorphic with one another and femaleness dominant. All females are heterozygotes, carrying recessive maleness, and producing male-bearing and female-bearing eggs in equal numbers; all males are homozygous, carrying only maleness and producing only male-bearing spermatozoa.

The history of the moth has a host of interesting Mendelian

points; and the suggestion is that the law may carry farther than even the Mendelians—who had things all their own way at Dublin—hoped. However, after all, the Darwinians had the last word. In “the division of spoils” a sum of £30 was allotted to Mr. Francis Darwin for “the study of heredity.”

Professor Curie shared with the Abbé Mendel the honours of the meeting. Physicists are absorbed in radium and its ancestors; and happily one of the Presidents—not, however, of the Physical but of the Geological Section—had during the year made an immense advance in the investigation not of the properties, a knowledge of which has not been much advanced by recent investigation, but of the distribution of radium and uranium. It was very delightful, while Professor Joly was delivering his remarkable address, to see the youthful zeal and excitement of Sir Archibald Geikie, who afterwards heralded “the new epoch in geology.” Both the record of ascertained fact and the inferences make a quite new contribution to both geology and geography. The fact is now established that uranium is the primary source of the supply of radium.

The recent discovery of ionium by Boltwood serves to link uranium and radium, and explains why it was that those who sought for radium as the immediate offspring of uranium found the latter apparently unproductive, the actual relation of uranium to radium being that of grandparent. But even were we without this connected knowledge, the fact of the invariable occurrence in Nature of these elements, not only in association but in a quantitative relationship, can only be explained on a genetic connection between the two. This evidence, mainly due to the work of Boltwood, when examined in detail, becomes overwhelmingly convincing.

Thus it is to uranium that we look for the continuance of the supplies of radium. In it we find an all but eternal source. The fraction of this substance which decays each year, or rather is transformed to a lower atomic weight, is measured in tens of thousands of millionths; so that the uranium of the earth one hundred million years ago was hardly more than one per cent. greater in mass than it is to-day.

The supply of uranium near the surface of the earth is surprisingly large. It might be expected that

the concentration of the heaviest element known to us (uranium) at the surface of the earth is just what we would not have expected. Yet a simple enough explanation may be at hand in

the heat-producing capacity of that substance. If it was originally scattered through the earth-stuff, not in a uniform distribution, but to some extent concentrated fortuitously in a manner depending on the origin of terrestrial ingredients, then these radio-active nuclei, heating and expanding beyond the capacity of surrounding materials, would rise to the surface of a world in which convective actions were still possible, and, very conceivably, even after such conditions had ceased to be general; and in this way the surface materials would become richer than the interior.

Taken all together, the case stands thus as regards the earth. We are assured of radium as a widely distributed surface material, and to such depths as we can penetrate. By inference from the presence of radium in meteoric substances, and its very probable presence in the sun, from which the whole of terrestrial stuff probably originated, as well as by the inherent likelihood that every element at the surface is in some measure distributed throughout the entire mass, we arrive at the conclusion that radium is indeed a universal terrestrial constituent.

The dependent question then confronts us—Are we living on a world heated throughout by radio-thermal actions? This question—one of the most interesting which has originated in the discovery that internal atomic changes may prove a source of heat—can only be answered (if it can be answered) by the facts of geological science.

It is very unlikely that uranium is entirely absent from the centre. If it is present it follows that the central parts of the earth, which are, so to speak, blanketed off from the surface, are growing hotter. From this conclusion Prof. Joly saw no escape, although we can adduce the evidence of certain surface-phenomena to show that the rise in temperature during geological time must be small or its effects in some manner kept under control. In a word, whether we assume that the whole heat-loss of the earth is now being made good by the radio-active heating or not, we find, on any probable value of the conductivity, a central core almost protected from loss by the immense mass of heated material interposed between it and the surface, and within this core very probably a continuous source of heat. It is hard to set aside any of the premisses of this argument.

That is one point. Another is the effect of this heat, which will be greater in certain areas and strains of detrited matter, on the surface of the earth. It is granted that the deposition of sediments somehow or other involves their ultimate upheaval. Of this phenomenon we have had no satisfactory explanation.

Professor Joly suggests a new one, which especially pleased the most veteran of the famous geologists present. "We see," he said, "in every case that only after great thicknesses of sediments have accumulated is the upheaval brought about. This is a feature which must enter as an essential condition into whatever explanation we propose to offer."

Following up the idea that the sought-for instability is referable to radio-thermal actions, he attempted an estimate of the rise of temperature in one of these sedimentary areas, and from its amount and also its irregularity drew the inference that

We have in these effects an intervention of radium in the dynamics of the earth's crust, which must have influenced the entire history of our globe, and which affords a key to the instability of the crust. For after the events of mountain-building are accomplished, stability is not attained, but in presence of the forces of denudation the whole sequence of events has to commence over again. Every fresh accession of snow to the firn, every passing cloud contributing its small addition to the torrent, assists to spread out once more on the floor of the ocean the heat-producing substance. With this rhythmic succession of events appear bound up those positive or negative movements of the strand which cover and uncover the continents, and have swayed the entire course of evolution of terrestrial life.

The adhesion of even the most polemical geologists—and they are still not above the Truthful James attitude to Old Red Sandstone—was expressed with surprising unanimity at a long discussion held on the last day of the Dublin meeting on mountain-building. Professor Joly himself said nothing about radium, but several of those who had spent the week-end in thinking over the revolution in their science had found all manner of new uses for it. The most ingenious, perhaps, was a suggestion that since the maximum of sedimentary deposits, and therefore of radium, was likely to be found at the edge of the big southern seas, the resulting heat would necessarily increase instability at that point, and so in the sequel produce such great upheavals as the Rockies.

Perhaps both Mendelism and radium are in danger of being over-exploited. They appear even in the popular press—which recently has grown quite affectionate towards the British Association—and are hailed as the great mystery-probers of the age. But both in their spheres have added immensely to the zest of physical and biological investigation; and in spite of the

critics, curiously silent at Dublin, Mendel may claim comparison, if at a distance, with Curie.

Possibly the air of Dublin had also something to do with the very political energy of many of the debates. Mr. T. W. Russell had a most characteristic round with Sir Horace Plunkett, who made a model President of a section, and his polemical example was followed elsewhere. Indeed, as critics of Government in general, the Presidents themselves set a vigorous example.

Professor Kipping, following closely the example of Prof. Meldola in his Presidential address to the Chemical Society, foretold the extinction of British manufactures of many sorts if research work was neglected by the State in the development of its business; and he had an ingenious plan for "the evolution of that otherwise almost unattainable being, the man thoroughly trained both in science and practice." His idea was an interchange between the university and the manufactory under his plan. The research students of the teaching institution, engaged on a given problem for a manufacturer, would be allowed to study its practical aspects in the works; on the other hand, work-chemists, with considerable practical experience, would be granted permission to proceed to the university laboratory, where they would study the problem with the assistance of the highest scientific knowledge, and acquire further training in the methods of research.

Combinations such as these could hardly fail to lead to valuable results, which would form the subject of patents; the monopolies thus acquired would place the manufacturers in a favourable position, and the revival of our chemical industries would follow in due course. There is nothing utopian in this scheme, and there are no great initial difficulties to be overcome; it may be set in operation by the manufacturer, and possibly also by the worker in pure science. Reading between the lines certain records which have recently appeared in the science journals and the patent lists, it may even be inferred that such arrangements are already in force in one of our large industrial centres.

His paper was full of admirable advice, but the weakness of all the proselytising papers at the British Association is that they do not reach the right sources. Scientific papers fall presumably on congenial soil; social and political papers never

even reach the seed-ground for which they are intended, and without which they cannot develop.

Sometimes, perhaps, this is a good thing. Society, as represented by teetotalers and vegetarians, and Government as represented by the Home Office, would not be induced to an admiration of science as a practical staff by the Utopian logic of Professor Ridgeway, who considers alcohol a necessary infliction of our climate.

The Education section again, which gathers its chief strength from the ubiquitous energy of Prof. Armstrong, does not, outside his efforts, pretend to be scientific in any proper sense of the word; and in spite of the excellence and usefulness of the debates, the cranks fly here like midges to a cuckoo-pint. The chemists talked education, the agriculturists talked education, the geologists talked education; and the chief use of a very popular section disappears completely, if more strictly scientific sections do not recognise its claim to the subject and at the same time free themselves of a burdensome duty. Efforts have multiplied to make the Association more and more popular, but after all the people are much more interested in real contributions to scientific knowledge than in discursive speeches.

A very informative struggle on this definition of the popular was to be traced in the sub-section—which will probably soon become Section M—devoted to Agriculture. Half the speakers wandered over the whole field—agricultural education, co-operation, housing, small holdings, and the rest. The other half endeavoured to bring science and agriculture into touch. Sir Horace Plunkett, the President, made a charming and a very earnest speech. But his attitude was wholly economic. His secretary was Mr. A. D. Hall, who, as head of Gilbert and Lawes' farm at Rothamsted, represented the most specialised agricultural science in this country. It is to be hoped that when Section M comes into being, the Rothamsted point of view will be taken. In this case, beyond all doubt, the more scientific attitude is much more popular than the so-called popular attitude, and only by attention to it will agriculture get much benefit from the Association.

THE EVOLUTION OF MAN

(A Paper read before the Mendelian Society)

By J T CUNNINGHAM, M A, F Z S

THE present paper is an attempt to consider some of the modern conceptions of biology in relation to the human species. With regard to species in general the Darwinian theory assumed that the differences between species were differences of adaptation, that specific characters were useful, that species were adapted to different modes of life. It has, on the other hand, been maintained by later zoologists that in the vast majority of specific characters there is no evidence of such utility, or of correlation with useful characters, and the pure Darwinian doctrine is now held by a few pious disciples of Darwin as an article of faith rather than as a scientific conclusion. The most eminent systematists distinguish now, as those of pre-Darwinian days did, between diagnostic characters, which are of chief systematic value, and adaptive characters, which for purposes of classification are often rather misleading than significant. The more useless a character is the more valuable it is as an indication of affinity. One modern school of evolutionists, recognising the uselessness of diagnostic characters, holds that they have not been evolved by selection but have arisen spontaneously as mutations; and, with the usual tendency to carry a doctrine to extremes, they maintain that all characters are independent of utility, that all arose as mutations. The American investigator, Dr. T. H. Morgan, has published a book specially devoted to this doctrine, in which he endeavours to show that adaptations do not really exist, that mutations have occurred which could only survive under special conditions of life, which in some cases the modified creatures have found, so that habits have been determined by structure, not structure by habits. Thus, in the short period of half a century, we have had the swing of the pendulum of biological opinion from one extreme to another, from the belief that all characters were adaptive or useful to the belief that none were adaptive. In the meantime the common-sense view has

persisted, that some characters are useful and some are not, and that the former are easily modified by conditions of life, the latter unaffected by such conditions. It must, at any rate, be admitted that usually in studying any group of animals we can certainly distinguish between characters which have no visible relation to the maintenance of life, and others which are necessary or advantageous to that purpose, and it is therefore possible to consider the origin of these two kinds of characters separately.

The human species, in spite of the attention devoted to anthropology, and although it is to us the most familiar species, has, perhaps, been less studied from the zoological point of view than any other. It is also from this point of view the most difficult, partly because it is our own species, and we cannot get far enough away from it to see it in true perspective; partly because it has had such an exceptional history, having spread over the whole earth and become largely independent of physical conditions, that is to say it has attained to a great extent the power of making artificially uniform conditions which render it independent of differences of climate, geographical features, and differences of fauna and flora in different habitats. The first question to consider is whether man is a single species or several, and what is his relation to other species. This question, as well as most of the others which I propose to consider in this paper, has been discussed with his usual thoroughness and judgment by Darwin in his *Descent of Man*, so that I am really only trying to see whether we know any more about these problems than Darwin taught us.

The chief peculiarities of man, as compared with his nearest allies, the anthropoid apes, are all adaptive and useful characters, namely the erect position, the structure of the hand and foot, and the faculty of articulate speech. Associated with the possession of language are the size and differentiation of the brain, especially of the cerebral hemispheres, and the correlated size and shape of the cranium. The reduction of the jaws, teeth, and face generally is also a characteristic feature, and adaptive to the diminution in the use made of the jaws and teeth in feeding and fighting. If man is regarded as a single species then he affords a conspicuous instance against the doctrine that specific characters are not adaptations, but it must be remembered that the contention is not that no specific characters are adaptive, but that in a vast number of cases several species

are distinguished and named which live in the same district under the same conditions, and that where they live in different habitats there is no evidence that the characters correspond with differences in the mode of life. On the other hand, there is no reason why a single species should not become adapted to some peculiar mode of life, but then it would be a matter of opinion among systematists whether it should not be placed in a separate genus.

Before proceeding further with this part of the subject it is interesting to consider the origin and nature of these adaptations. While others have been disputing whether acquired characters are ever inherited and whether adaptations are due to the inheritance of acquired characters, Dr. Archdall Reid has made the brilliant discovery that such adaptations as those which distinguish man from the anthropoid apes are not inherited at all but are acquired by every individual in the course of his development. Inborn or congenital characters, he says, are developed by the stimulus of nutrition alone, acquired characters are developed by the stimulus of use. Modifications acquired as a result of use and disuse are clearly never transmitted, because they never develop except in response to the same stimulation as in the parent:¹ "Plainly then that which is transmitted to the infant is not the modification but only the power of acquiring the modification under similar circumstances—a power which has undergone such an evolution in high animal organisms that in man, for instance, nearly all the developmental changes which occur between infancy and manhood are attributable to it." Now while it must be admitted that it is very important to ascertain how far characters are developed entirely as the result of the constitution of the germ-plasm and how far they require an appropriate stimulus, I think Dr. Reid attributes excessive importance to the latter factor. We know that a child only learns to speak by hearing speech, but we also know that monkeys and dogs do not learn to speak under the same conditions. The difference, therefore, between man and his nearest allies is not in acquirement but in hereditary constitution. Indeed, Dr. Reid admits as much, but he puts the fact in other words, and here, as in much else that he has written, it seems to me that he imagines he has discovered something new when he has expressed what was known before in different

¹ *Principles of Heredity*, 2nd Edit., London, 1906, p. 35.

and unnecessarily abundant language. He says that since "Nature" has endowed animals with the power of making not all possible acquirements but only certain fixed acquirements that are commonly useful to the species, therefore species differ not only in characters which are inborn but also in those which are acquired; for instance, the fore-limbs of both ox and man grow greatly in response to use, but the lines of growth are very different. Exactly. Of course Dr. Reid assumes that the difference in the power of making acquirements is due to natural selection. The power of growth in response to exercise resides, he says, not especially in the parts which are most used, as joints, teeth, or tongue, but in the parts in which it is most useful; in other words in those parts where it has been evolved, not by use, but by natural selection. It would be difficult to compress a greater number of fallacies into such few words. The chief fallacy lies in the word use; use of a muscle means contraction, and contraction causes growth of muscle; but it is obvious that joints do not contract and that a joint has no size. The fact that joints can be developed by use is proved by their actual formation occasionally in neglected fractures. It is also obvious that the tongue, being a muscular organ, is developed by exercise, not merely in absolute size, but in the complexity and precision of its movements, as in the muscles of the hand; otherwise we could not learn to speak. In fact, it is precisely because the power of acquiring certain structural adaptations resides in those parts which are used for certain purposes that Lamarckians conclude that the power to acquire and the acquirement are due to the same causes, in other words that the hereditary or congenital factor and the acquired factor in adaptations are both due to external stimuli. The contrary view is mere assertion based on no evidence. What evidence, for instance, is there that the ancestor of man possessed a variation in the power of acquiring the upright position, independently of the attempt to walk on his hind legs?

Dr. Reid supposes that this power of making acquirements is greatest among the higher animals, and little or not at all present in the lower animals and plants. He instances the frog, and expresses his belief that a tadpole enclosed in a hole or crevice, if supplied with food, would develop into a perfect frog, and that this is possibly the explanation of those cases reported in the newspapers from time to time of perfect frogs found

enclosed in stone in quarries. It is unnecessary to discuss seriously this suggestion: it will be sufficient to consider how much foundation there is for the dictum of Dr. Reid, that the frog's body gains nothing from use, and his mind almost nothing from experience. This implies that the metamorphosis is entirely due to heredity, and not at all to stimulation. It has been proved, on the contrary, that aquatic larvæ of Amphibians can be made to retain the larval state by forcing them to breathe in the water, and not allowing them to breathe air, so that in this case, as in many others, the development is partly due to acquirement in Dr. Reid's sense of the term. Dr. Reid contrasts with the supposed development of the frog, the alleged fact that if the limb of an infant be locked by paralysis or by a joint disease, it does not develop into an adult limb, but there is every reason to believe that the same statement would be true of the frog.

We must conclude then that man differs from the anthropoid apes chiefly in adaptational characters, and that these characters are inborn or congenital; they are congenital in two senses: first, in the sense that they develop to a certain degree under what Dr. Reid calls the stimulus of nutrition, by which he means nutrition, moisture, heat, and oxygen, the essential conditions of all development and all life;¹ secondly, that they attain their adult development from an hereditary tendency to certain modes of use and function, and from a degree of exercise which would not produce the same development in any other species. The new-born infant differs from the adult man, but it also differs from the new-born ape in all essential human characters, and that adult has acquired structural peculiarities which no ape could possibly acquire from any stimuli in its own life-time. Obviously these are not merely specific characters, and man is not merely a species of a wider genus. Adaptational differences are characteristic among other animals of a genus, or of a family, or of larger groups. For example, among the mammals the Orders are distinguished by differences of adaptation, *e.g.* the Cheiroptera and Carnivora; but within a

¹ The antithesis on which Dr. Reid insists between characters developed under the stimulus of nutrition and those developed under the stimulus of use is fallacious and misleading. Nutrition, moisture, heat, and oxygen are necessary conditions of all life: different characters develop under these same conditions, they determine the development of the organism, not of the characters; whereas a particular mode of use determines a particular character.

single Order a Family may be so separated as in the case of the Mole family. It is not easy to find a genus so distinguished. Man thus appears to have the rank of a family. The condition of the hair in man might possibly be regarded as a diagnostic character which is not adaptive; if the absence of hair on the body be explained by uselessness, still the special development on the head looks like a non-adaptive feature. It is difficult then to regard man as merely a genus of anthropoid apes.

On the other hand, we do not find that man can obviously be divided into a number of distinct species as other families of mammals can, or as even a genus can be divided. There are distinct races of man, and the question is whether these correspond with species among other animals. To discuss this question we have to consider the diagnostic characters of these races. Darwin considers them very carefully in the work I have already mentioned, and comes to two remarkable conclusions, which are of chief importance in relation to the object of this paper: first, that these characters graduate into each other so that the races cannot be absolutely defined; secondly, that they are in no sense adaptational. He says that, so far as we can judge, none of the differences between the races of man are of any direct or special service to him, nor can they be accounted for in a satisfactory manner by the direct action of the conditions of life, nor by use and disuse, nor through the principle of correlation. He then proceeds to inquire whether they can be explained by sexual selection. He concludes that this process will not explain all the differences, but that there is a residuum which must, provisionally at least, be regarded as due to spontaneous variations, which have become constant and general without selection. Thus we find Darwin in this case compelled to adopt the view which in my opinion still holds good in man as in other animals, that there are two categories of characters, namely the adaptive and the non-adaptive. The latter are of the same kind as those which are called mutations by modern biologists, while the former, in my opinion, are directly due to stimuli. Where the stimulus is functional, the modification is such as to render organs and structures more fitted for the functions; but certain conditions may produce a direct result which has no connection with function, and which is therefore not in the ordinary sense useful or adaptive: for example, the absence of the stimulus of light

causes the absence of pigment from the lower sides of flat-fishes, but this character is neither useful nor adaptive. The real distinction between the two kinds of characters, according to my views, is that those of one kind are due to external stimulation, those of the other kind are independent of external causes; the latter are mutations, the former may be called modifications.

Huxley's classification of the races of mankind is a somewhat simple one. He divides them into two primary divisions, resembling perhaps genera, namely the Ulotrichi with woolly hair, and the Leiotrichi with straight hair. The Leiotrichi he subdivides into four groups: the Australoid, the Mongoloid, the Xanthochroic or fair whites, and the Melanochroi or dark whites. The characters in which the races differ are colour of skin, hair, and eyes, shape of cranium, whether brachycephalic or dolichocephalic, character of hair, projection of jaws, shape of features, especially nose and eye-apertures.

The negro race is one of the most distinctly marked: its characters being black-brown skin, woolly hair, prognathous, brachycephalic skull, thick out-turned lips, flat nose. The type is most perfectly developed in equatorial Africa as in Guinea; to the north it has crossed with the Berbers, on the east with the Arabs; to the south it shows reduced characters apparently without crossing. The Bushmen and Hottentots, while apparently belonging to the negro stock, are much lighter in colour, and this is some evidence that the black of the negro is originally due to the tropical sun. In the East Indian Archipelago is a type allied to the negro, but not identical, extending through the region called from its presence Melanesia, from Papua to Fiji. These have woolly hair, but the brow ridges and the nose are more prominent than in the negro; the colour varies from black to chocolate. The Tasmanians, now extinct, are stated to have been an isolated colony of this race. The Andaman islanders form a connecting link between the eastern blacks and the negroes, but although they have frizzled hair, which they shave off, they are brachycephalic, less prognathous than the negro, and the nose is narrower. The Australians are brown in colour with wavy black hair, dolichocephalic, having prognathous skulls with well-developed brows, and wide but not flat noses. Allied types are believed to occur in India, namely the hill-tribes, descended from the

primitive inhabitants, and in North-east and North Africa in the Nubians, Berbers, and the ancient Egyptians.

The Mongoloid race seems to occur in its purest state in Central and Northern Asia, and its features are: yellowish colour, long black straight hair, high cheek-bones, short nose, and, especially, slanting eye-apertures. The allied races are of immense extent, and the features in many considerably modified either by variation or inter-crossing. The skulls in some of these are extremely dolichocephalic. In Asia the Mongoloid type is extended by the Japanese, Chinese, Siamese; in Europe its invasions are represented by the Turks, Finns, Lapps, and Hungarians. The Malay race seems to be a branch of the Mongoloid and extends over Java, Sumatra, Borneo, and can be traced to New Zealand, and throughout Micronesia and Polynesia. It is supposed that the Maoris and the islanders of the Pacific, differing considerably from each other in characters, have arisen chiefly from crossing between a race allied to the Malays and the darker Melanesians. The inhabitants of the whole of America, north and south, seem to belong to one main type supposed to have been derived from the Mongolian. The uniformity of the American Indians, as compared with the diversity of types in the old world, is one of the most striking facts in anthropology, and is most probably explained by the view that America was populated from a single race, the Mongolian, from Asia, within a period so comparatively recent that no great divergence has been developed. Lastly we have the white men whose home is chiefly in Europe, and who include a great variety of subordinate types.

The type with blonde hair and blue eyes is found chiefly in the north—*e.g.* in Scandinavia and North Germany—but representatives of it are found in North Africa and in Western Asia. There can be little doubt that this is a pure type; but Huxley suggests that the Melanochroi originated from the mixture of the Xanthochroi and Australoids of India and North Africa.

I can only attempt to offer a few suggestions on the characters which distinguish the races thus rapidly surveyed. Mendelians will assume that they are all simply mutations, but this does not seem to me to be a reasonable conclusion. Some of them, as in other groups of animals, are differences of degree in those adaptive characters which distinguish man from the apes—for example, prognathism and the size of the cranium and

brain. The decrease in prognathism is obviously associated with the degree and duration of civilisation, so that this character in negroes and Australians is adaptive. Perhaps the same may be said of the extreme dolichocephaly associated with a sloping forehead in the same races; but we cannot say that the men with the shortest skulls are the most civilised, for some of the least civilised Mongolians are more brachycephalic than the English. In my opinion there is good evidence that dark or black skin-colour is correlated with the light and heat of the tropical sun. It may be objected to this that the American Indians of the tropics are not black like the negroes; but I believe they are considerably darker than those of the north. At any rate, we have the following facts: that no very dark race occurs in temperate climes either north or south, that the negroes of equatorial Africa are distinctly darker than the Bantus and Hottentots of South Africa, and that the negroes of North America have become lighter since their importation.

I have sometimes thought that perhaps modifications due to stimuli might differ from mutations in not exhibiting Mendelian segregation; but the evidence, so far as we have any, is contradictory, for, while crossing of negroes with whites always gives intermediates in all degrees of mixture, we have a constantly repeated segregation when dark whites and fair whites interbreed. Eimer mentions this as especially conspicuous in South German villages, where the inhabitants continually intermarry, and yet pure blondes and dark children occur constantly in the same family. This may not be typical Mendelism, for we do not find dominance in the heterozygote; it may be a case of alternate dominance, and we do not know whether segregation takes place.¹ There are, however, many race characters which seem to be evidently mutations, since there is no evidence that they are useful or due to external conditions. As examples of these, we may mention the character of the hair with regard to curling, the direction of the eye-aperture, the prominence of the nose. We have little precise evidence concerning Mendelian inheritance in these. Mr. Mudge published lately some data concerning the inheritance of such features in crosses between Canadian Indians and Europeans;

¹ Mr. C. C. Hurst has recently shown that dark eyes of any shade are dominant to blue eyes, and that the two characters segregate in Mendelian fashion (*Proc. Roy. Soc.* 1908).

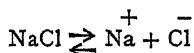
but it seemed to me that he found all the Indian characters segregating together in one individual, and that this could only occur in a much smaller proportion of cases than he stated.¹ His evidence would have been more convincing if he had dealt with single, marked features, and proved that they segregated. In negro crosses we have no satisfactory evidence of segregation in any character, whether adaptive or otherwise.

A word or two may be devoted to the consideration of Darwin's suggestion that sexual selection may account for the non-adaptive characters of human races. I have shown that where the characters are confined to one sex, selection cannot be the cause of this limitation. Where a character is already unisexual, however, it may vary and remain unisexual—as, for instance, in the human beard. The question then is whether selection by the female is required to account for a difference in the beard, or whether the mutation might not establish itself without selection. In deer the antlers differ in different species in size and shape, and it could scarcely be suggested that the particular size and shape in a given species was due to the fact that they were the best for fighting, or the most admired by the female. But sexual selection might affect characters which were not limited in inheritance—for example, the black of negroes might be due to the preference by either or both sexes for the darkest skin, but this is not a probable view.

In a short paper like the present, I can only give a very imperfect outline of the subject, but I hope I have said enough to show that anthropology requires to be reinvestigated from modern points of view. My own provisional conclusions are that man affords an example of a single species which has started a new group, which might become a genus or family. Other genera or families may have originated in this way by a single species adopting a new mode of life. The evidence does not seem to me to support the view that all human characters, adaptive and non-adaptive, can be regarded as mutations independent in their origin of habits or functional or other stimuli. The evidence seems to me to agree with the view I take of animals in general—that adaptive characters are due, not to selection, but to the effects of functional and physical stimulation, and that diagnostic characters are not adaptive, and therefore not due to selection, but to blastogenic variation.

¹ *Nature*, November 7, 1907, p. 9.

dissociation of the salt had been overcome, the new theory was so simple as to lend itself readily to dogmatic treatment; its advocates scarcely seemed able to realise that so excellent a theory might possess limitations, or that facts urged in its support might be capable of alternative interpretations. The ionisation of a salt in dilute solutions was treated as a spontaneous process depending mainly on volume considerations, and was no longer regarded as a chemical equilibrium in which the presence of an excess of solvent was the dominant factor. The text-books written in exposition of the new theory of solutions scarcely mentioned the solvent except as a mere diluent, and equations such as



were put forward as expressing all the essential features of the process of ionisation. The later history of the theory, and its development into a maturer and more reasonable (even if less dogmatic) form, has indeed consisted almost entirely in a reversion to the earlier idea that "perhaps the active part is a compound of [solute] and solvent," and that "the three constituent parts of the solution form a system in chemical equilibrium such that dilution increases the active and diminishes the inactive portion."

PHYSICAL THEORIES OF THE ACTION OF THE SOLVENT

The earliest attempts to account for the widely different conductivities of the same solute when dissolved in different solvents—or in the same solvent at different temperatures—were based on purely physical considerations, amongst which the viscosity or fluidity of the solution has always played a prominent part (see, for example, Wiedemann, *Pogg. Ann.* 1856, 99, 229). It was one of the conspicuous merits of the theory of Arrhenius that it resolved the conductivity of a solution into two factors, ionisation and mobility, which could be determined experimentally and submitted to separate examination. The idea of separate ionic mobilities for anion and kathion, it should be noted, had been introduced by Kohlrausch some years previously (*Göttingen Nachrichten*, 1876, 213), but he considered that the whole of the solute was directly concerned in electrolysis, and therefore arranged his values so that the sum of the

mobilities of kation and anion was equal to the molecular conductivity

$$\lambda_n = u'' + v''$$

for the given dilution. This method of representation undoubtedly possesses certain advantages, in view of the fact that the coefficient of ionisation cannot be accurately determined except in the most dilute solutions. In moderately dilute solutions a fair approximation can indeed be reached, but the coefficients for concentrated solutions are absolutely unknown quantities, and in the case of fused salts no method has yet been devised for determining even approximately the value of the coefficient, so that it is actually impossible at the present time to say whether the whole or only a part of the salt is directly concerned in electrolysis. Arrhenius's method of dealing with ionic mobilities differs from that of Kohlrausch in assuming that the ionic mobilities are substantially constant in dilute solutions, and that the decrease of molecular conductivity when the concentration is increased is due to decreased ionisation, and not to decreased mobility. Thus he wrote

$$\lambda \propto u \propto + v \propto$$

but (in place of $\lambda_n = u_n + v_n$)

$$\lambda_n = a(u \propto + v \propto)$$

—a novel method of representation that has proved to be of very great advantage in the study of the dilute solutions which have hitherto formed the principal subjects of investigation.

In a few exceptional cases, such as that of heated glass (Bousfield and Lowry, *Proc. Roy. Soc.* 1902, **71**, 45) or concentrated caustic soda (*ibid. Phil. Trans.* 1905, **204**, 253-322), the mobility of the ions may be the most important factor in determining the conductivity of an electrolyte, but in all the more ordinary solutions the variations of mobility are relatively small. The factors governing them (ionic size, viscosity, etc.), which are discussed below, are therefore of secondary importance compared with the fundamental problems associated with the coefficient of ionisation—as, for example, why a solution of hydrogen chloride in water should be a good conductor, a solution in alcohol a moderate conductor, a solution in ether a very poor conductor, and a solution in benzene an excellent insulator.

In considering this problem, the theory put forward almost simultaneously by J. J. Thomson (*Phil. Mag.* 1893, v. **36**, 320)

and by Nernst (*Zeit. phys. Chem.* 1893, ii. 220), that the coefficient of ionisation depends mainly on the specific inductive capacity of the solvent, has much to commend it, both on the theoretical and on the experimental side. As a matter of theory, it was pointed out that the ordinary law governing the attraction between charged bodies

$$F = \frac{q_1 q_2}{\epsilon r^2}$$

shows that the force between them varies inversely as the specific inductive capacity ϵ of the medium; the attractive force between a pair of ions immersed in water as a dielectric would therefore be only one-eightieth of that in air or one-fortieth of that in a hydrocarbon medium such as benzene or petroleum. On the experimental side it is to be noted that the small group of substances which possess in a high degree the power of producing ionisation are for the most part substances of high specific inductive capacity, *e.g.*:

Water	specific inductive capacity, 80
Hydrogen cyanide	" " " 96
Ammonia	" " " 22

and that in homologous series the specific inductive capacity and the ionising power of the solvent decrease together as the molecular weight increases, *e.g.*:

Water	$\epsilon = 80$	Hydrogen cyanide	96	Methylamine	10
Methyl alcohol	35	Methyl cyanide	36	Ethylamine	6
Ethyl alcohol	26	Ethyl cyanide	26	Ammonia	22
Ethyl ether	4	Propyl cyanide	20		

Attention has frequently been called to the existence of exceptions to the Nernst-Thomson rule, but in many of the cases that have been quoted the comparison has been invalidated by the use of the molecular conductivity of a solution in place of its coefficient of ionisation as a measure of the ionising power of the solvent. Thus it has frequently been noticed that solutions in liquid ammonia at -37° have a higher electrical conductivity than the corresponding solutions in water at $+18^\circ$, although the specific inductive capacity is much higher in the latter solvent (Goodwin and Thompson, *Physical Review*, 1899, 8, 43); but the more recent experiments of Dutoit (*Zeit. Elektrochemie*, 1906, 12, 642) have shown that the coefficients of ionisation are relatively low, and that the high conductivity of the ammoniacal solutions is due to the exceptionally great mobility of the ions in this solvent, and not to any excess of

ionising power. Similar considerations may invalidate to a large extent the analogous criticisms of Schlundt (*Jour. Phys. Chem.* 1901, 5, 168), who has shown that the conductivity of silver nitrate is four times as great when dissolved in propionitrile as when dissolved in benzonitrile, in spite of the fact that the specific inductive capacities of the two solvents are practically equal, and that the same salt conducts better when dissolved in pyridine, s.i.c. 12, than when dissolved in butyronitrile, s.i.c. 20; it is quite possible that these differences may be due to a special ionising power of certain solvents when acting on particular salts (*e.g.* to a specific interaction between silver nitrate and pyridine which might not occur between silver nitrate and butyronitrile, or between potassium iodide and pyridine), but in the absence of data in reference to the ionic mobilities it is impossible to say how far the cases quoted may or may not constitute exceptions to the Nernst-Thomson rule.

The recent extensive experiments of Walden (*Zeit. phys. Chem.* 1906, 54, 131-230) on the electrical conductivity of tetraethylammonium iodide, $N(C_2H_5)_4I$, in a series of fifty organic solvents do not suffer from this limitation, as the observations were such that in almost every case the coefficients of ionisation could be calculated. The most important result of the investigation was to show that in a large range of instances *the dilution required to produce a given coefficient of ionisation in the iodide was inversely proportional to the cube of the specific inductive capacity of the solvent*. This result may be expressed mathematically as

$$e v^{\frac{1}{3}} = \text{constant, where } v = \text{volume containing one equivalent.}$$

and the following table shows the degree of concordance attained in one typical series:

	α	ϵ	v	$ev^{\frac{1}{3}}$
H ₂ O	0.83	80	32	254
CH ₃ NO ₂	"	40	200	234
C ₂ H ₅ O	"	39	200	230
CH ₃ CN		36	320	245
CH ₃ SCN		36	270	233
CH ₃ OH		32.5	400	240
C ₂ H ₅ CN		27.2	800	253
CH ₃ (CO.CH ₃) ₂	"	26.0	1000	260
O(CO.CH ₃) ₂	"	17.9	2000	226
C ₆ H ₅ .CH ₂ CN	"	16.7	3200	246
CH ₃ O.C ₆ H ₄ .CHO	"	15.5	3100	226
C ₆ H ₅ .CHO	"	14.5	4100	232

The variations in the products $ev\frac{1}{2}$ in the above table are probably within the limits of experimental error, and it is clear that, at least in the case of this particular solute, the Nernst-Thomson rule may have not only a qualitative but actually a quantitative significance. It is necessary, however, to call attention to the fact that the solute selected was not a typical salt, but a somewhat complex organic compound. Whilst, therefore, the simple relationship $ev\frac{1}{2} = f(a)$ may hold good in certain limiting cases, it is more than doubtful whether it would survive if applied to compounds exhibiting such strong individuality as is displayed by the metallic salts.¹

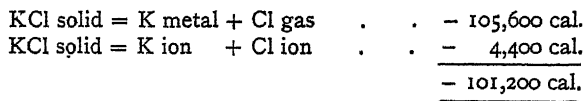
INADEQUACY OF THE PHYSICAL THEORY OF IONISATION. THE SOLVENT AS A SOURCE OF ENERGY

Having shown in the above discussion the important relationship which exists between specific inductive capacity and ionisation, it is necessary to add some criticisms of the physical explanation of the phenomena of electrolytic dissociation. In the first place it should be noted that the explanation is incomplete, in that no hint is given as to the origin of the different coefficients of ionisation for different solutes; the physical theory of the action of the solvent in electrolysis must therefore in any case be supplemented by a further theory—presumably chemical in character—in reference to the function of the solute. In the second place it should be noted that the mere weakening of the electrical forces between the ions does not afford an adequate explanation of the dissociation of the salt.

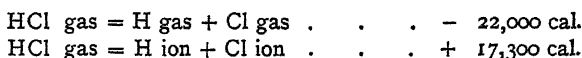
The latter point bears directly on the fundamental difficulty of the theory of electrolytic dissociation—the source of the energy required for ionisation—and is of such importance as to demand separate discussion. To take a concrete case: the formation of solid potassium chloride from its elements is accompanied by the liberation of 105,600 calories, and a corresponding amount of energy must be supplied if the salt is to be decomposed again into metallic potassium and gaseous chlorine. The dissolution of the salt in a large quantity of water is accompanied by an absorption of 4,400 calories, but the products, according to the theory of electrolytic dissociation, are simply potassium ions and chlorine ions. We are therefore presented

¹ Contrast, for example, the different behaviour of silver chloride and potassium chloride towards water and ammonia.

with the surprising contrast that whilst the dissociation of the salt into metal and gas involves the absorption of 105,600 calories, the dissociation into ions involves an absorption of 4,400 calories only.

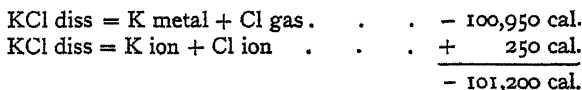


In the case of hydrogen chloride the figures are even more remarkable :

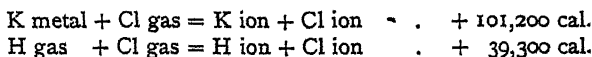


since the heat changes, although of similar magnitude, are actually opposite in sign.

The difficulty was accentuated when Arrhenius (*Zeit. phys. Chem.* 1889, 4, 104), by making use of van't Hoff's equation, was enabled to calculate from the temperature-variations of the coefficient of ionisation the thermal change involved in the ionisation of the dissolved salt. In the case of potassium chloride this amounted to a liberation of +250 calories,¹ so that the equations for the dissociation of the dissolved (but non-ionised) salt are



An alternative expression of the heat changes involved in ionisation is given by combining the above equations, *e.g.* :



From these equations it appears that the conversion of an element from its ordinary form into the ionised form involves the liberation of large quantities of heat, the sum of the heats of formation of a pair of ions being identical with the heat of formation of the dissolved salt as determined experimentally.²

From what source are these large quantities of energy

¹ In a subsequent paper (*Zeit. phys. Chem.* 1892, 9, 340) a small negative value was given, but the earlier value appears more probable in view of the fact that the coefficient of ionisation decreases when the temperature is raised.

² For a full discussion of the heats of formation of the ions see Bousfield and Lowry, *Trans. Faraday Soc.* 1907, 3, 129.

derived? Ostwald (*B.A. Report*, Leeds, 1890, p. 333) suggested that—

“the ions H and Cl are allotropic forms of the elements, similar to red and yellow phosphorus, and contain very different amounts of energy from those which they contain in their common state of hydrogen and chlorine gases.”

Arrhenius (*loc. cit.* 1889) expressed the same idea by quoting the case of oxygen and ozone as justifying the positive values which he had found for the heat liberated in electrolytic dissociation.

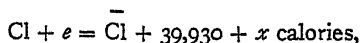
“One case of ordinary dissociation is known in which the decomposition is accompanied by liberation of heat. If two gram-molecules of ozone (O_3) are converted into three gram-molecules of Oxygen (O_2) the change is accompanied by the development of not less than 30,000 calories.”

But these analogies are rendered altogether invalid if the ions in question are merely single atoms electrically charged. The energy of an atom may be decreased by combination with other atoms, but an increase of energy would only be possible if the atoms were to exert an actual repulsion against which work could be done—a possibility that is altogether foreign to present-day chemical conceptions. Whilst, therefore, the conversion of one complex into another may be accompanied either by an absorption or by a liberation of energy, the dissociation of a molecule into single atoms must inevitably be endothermic.¹ The hydrogen or chlorine ions cannot, therefore, owe their superior stability to allotropy, if that somewhat vague term is interpreted as referring to differences of molecular structure such as those which exist in red and yellow phosphorus or in oxygen and ozone.

If then the ions exist in solution as single charged atoms they

¹ “If anything at all is certain about atoms, it is that the atoms in an elementary molecule are united very firmly together, and that therefore in separating them a very large absorption of heat would occur. To separate 2HCl into 2H and 2Cl would absorb far more heat than the $2 \times 22,000$ cal. which we know are absorbed in separating 2HCl into H_2 and Cl_2 . Yet the supporters of the dissociation theory would have us believe that this separation has actually taken place, not only without any absorption of heat, but actually with a development of $2 \times 17,300$ cal.; that is, that $2 \times 22,000 + 2 \times 17,300 + x$ cal. have been created, and that, too, through the intervention of the water, which has *ex hypothesi* no action whatever” (Pickering, “On the Theory of Solution,” *B.A. Report*, Leeds, 1890, p. 319).

must do so in virtue of the charge and not in virtue of their monatomic condition. This implies, if the theory of electrolytic dissociation be true, that enormous stores of energy must be available from the combination of the atom with the ionic charge of electricity. For instance, if Ostwald's value, -600 cal., be taken for the heat of formation of the hydrogen ion, that of the chlorine ion must be $39,930$ cal.; and even this figure does not represent the whole of the energy that must be postulated as arising from the union of the atom with its charge, since the conversion of chlorine gas into chlorine ions involves the disruption of the chlorine molecule as well as the electrification of the resulting atoms. We must therefore write

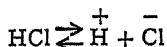


where x is the heat of formation of chlorine molecules from chlorine atoms.

How far then is it legitimate to draw on this hypothetical source of energy in explanation of the phenomenon of electrolytic dissociation? Attention may be directed in the first place to the behaviour of gases. If so much energy were available from the electrification of the atom the ionisation of a gas should take place spontaneously as an exothermic action quite independently of the presence of any solvent. Gaseous hydrogen chloride should therefore not only conduct the current but—in view of the exceedingly low viscosity of the gas—conduct it even more readily than in aqueous solution. Actually, however, the ionisation of a gas appears invariably to be an endothermic process, and one, moreover, that is only brought about to any marked extent when energy of a very high order of intensity is available, as for instance from radioactive matter (the most intense source of energy that is known) from Röntgen rays, or from ultra-violet light. Even these agents do not impart any high degree of conductivity to the gas, and the removal of the source of energy is at once followed by a more or less rapid “decay of ionisation.” The idea that the electrification of the atom may be regarded as a source of energy is therefore one that receives conspicuously little support from the experimental behaviour of gases.

It is also not upheld by the behaviour of solutions. If, for instance, the only difference between water and benzene consisted in their widely divergent specific inductive capacities, it might be expected that ionisation—if a spontaneous process—

would proceed equally rapidly in both media; the decay of ionisation, on the other hand, might be assumed to depend on the electrical forces between the ions, and therefore on the inductive capacity of the medium. The equation



would therefore apply equally well in both cases, the only essential difference being that the reverse action would be, say, forty times as rapid in benzene as in water. This would lead to a reduction of the coefficient of ionisation from 80 per cent. in water to, say, 2 per cent. in benzene. As, however, the solutions in benzene are excellent insulators, whilst the aqueous solutions are excellent conductors, it is evident that the difference in ionising properties is out of all proportion to the difference in inductive capacities, and that the solvent must not merely "permit" ionisation but exert a positive action in promoting it.

In view of the difficulties attaching to the theory that the electrification of the atoms may be regarded as a source of energy, it is of interest to follow out the alternative supposition that the ions may be treated as conductors which require an expenditure of energy for charging. This leads to the expression

$$E = 27\pi \times 10^9 \times \frac{l}{\epsilon} \text{ where } l = \text{mobility, } \epsilon = \text{s.i.c.}$$

for the energy of electrification of one gram-equivalent of ions when treated as spherical condensers, the radius of which may be determined from their mobility by the application of Stokes Law.¹

This result, which is independent of any assumption as to the dimensions of the atom or the absolute magnitude of the ionic charge, may also be expressed in heat units by making use of Joule's equivalent, when the above expression becomes

$$E = \frac{27\pi \times 10^9}{4 \cdot 2 \times 10^7} \frac{l}{\epsilon} = 2000 \frac{l}{\epsilon} \text{ calories.}$$

In water, taking $\epsilon = 80$, typical values for the energy absorbed in electrification are

H^+	Na^+
7950 cal.	1110 cal.
Cl^-	K^+
1650 cal.	1640 cal.

¹ For the details of the calculation and a fuller discussion reference may be made to the original paper (Bousfield and Lowry, *Trans. Faraday Soc.* 1907, 3, 126-128).

The assumption made in the preceding paragraph makes no attempt to account for the origin of the energy in ionisation, but rather brings out more strongly than ever the magnitude of the forces resisting it. It is, however, important to notice that the opposing electrical forces are shown to be inversely proportional to the inductive capacity of the medium, so that whilst the electrification of a gram-equivalent of hydrogen chloride in water would demand the expenditure of 9,600 calories, the same process carried out in benzene (assuming, provisionally, equal values for the ionic mobilities) would demand an absorption of 400,000 calories! Whilst, therefore, it is necessary in every case to seek for some force powerful enough to overcome the natural attraction of the ions, the task is rendered practically hopeless in the case of a medium of low inductive capacity by reason of the enormous quantity of energy used up in the process of electrification, apart altogether from the *chemical* affinity of the atoms.

Whatever differences of opinion may exist as to details, there can be no doubt that, broadly speaking, it is to the solvent that we must look for the main source of the energy in ionisation.¹ It is, moreover, more than probable that this energy is rendered available by a chemical interaction between the solvent and solute. The theories which associate ionisation with the polymerisation of the solvent or with the presence in the solvent molecules of unsaturated atoms such as divalent oxygen or trivalent nitrogen, do not in themselves afford any real explanation of the cause of ionisation; observations of this kind are of the greatest importance as indicating those properties of the solvent which are most likely to come into operation when it interacts with the solute to produce an electrolyte, but from the point of view of energy it is not enough that the molecules of the solvent should play the part of an onlooker—however sympathetic—they must themselves take part in the struggle and contribute their share in the hard work of ionisation.

¹ "It is acknowledged that different solvents have different powers of ionising a given substance, thereby conclusively proving that the function of the solvent cannot be properly described as merely giving the ions space to resolve themselves. And those who speak so acknowledge that it is only an analogy, or a *façon de parler*. But it seems a very misleading analogy, which leaves out the really active part that the solvent plays, and attributes to it a purely passive part" (Fitzgerald, *B.A. Report*, Leeds, 1890, p. 327).

DEVELOPMENT OF THE CHEMICAL THEORY OF IONISATION

The first suggestion that ionisation depends, not on the physical properties of the solvent, but on a chemical equilibrium between solvent and solute, was made by Arrhenius in his 1883 memoir as quoted above. This suggestion possesses obvious merits, but it is only in the most recent years that its advantages have been generally recognised. These may be broadly stated as follows:

(a) If, as Arrhenius suggested might be the case, "the active part [of the electrolyte] is a compound of the [solute] and solvent," the proportion of solute present in the active form will depend on the relative concentrations of solvent and solute, and will reach a maximum when the solvent is present in large excess. The equilibrium will necessarily be "such that dilution increases the active and diminishes the inactive part," and the effect of dilution in increasing ionisation will therefore receive at least a qualitative explanation if the chemical theory of ionisation is adopted.

(b) There is abundant evidence to show that the tendency to form hydrates rapidly decreases when the temperature is raised. If ionisation depends on the formation of similar compounds, it is clear that the ionising properties of a solvent must decrease when the temperature is raised, and may be expected to vanish at the critical temperature of the solution when the solute is no longer able to retain the solvent in the liquid form. The conductivity of a composite electrolyte usually increases with the temperature, but this is due to the increased mobility of the ions, and in every case is accompanied by a decrease in the coefficient of ionisation, which becomes more and more pronounced until at the critical temperature conductivity and ionisation disappear together.¹ The effect of temperature on ionisation is therefore entirely in accord with the requirements of the chemical or "hydrate" theory of ionisation.

(c) The part played by residual affinity in promoting ionisation also receives a satisfactory explanation if it is recognised that the process involves the formation of additive compounds of solvent and solute. Such compounds are not produced in the case of solvents which are fully saturated, but water and

¹ For a discussion of the influence of temperature on conductivity see Bousfield and Lowry, *Proc. Roy. Soc.* 1902, **71**, 42.

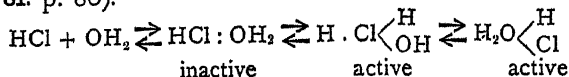
ammonia—both of which contain an atom which is capable of assuming a higher valency—possess in a remarkable degree the power of combining with salts, and are amongst the best of the ionising solvents. On the other hand, the chemical theory of ionisation recognises to the full the importance of the specific properties of the solute, the mutual affinity between which and the solvent is the determining factor in the production of active electrolytic complexes.

(d) It is evident that solvents which are capable of combining with a large range of solutes are likely also to possess the property of forming loose complexes composed entirely of solvent molecules. This is not, however, a direct cause or necessary condition of ionisation, and whilst the majority of ionising solvents undoubtedly belong to the group of associated liquids, there is no reason for assuming that electrolysis can only occur in solvents of this group. Sulphur dioxide, for instance, which dissolves a number of iodides with the formation of conducting solutions, possesses the property of forming additive compounds, but appears itself to be a non-associated liquid. (Walden, *Zeit. phys. Chem.* 1902, **39**, 513-96; Walden und Centnerszwer, *ibid.* 1903, **42**, 432-68.)

The exact nature of the chemical equilibrium involved in the process of ionisation has been the subject of much divergence of opinion. Arrhenius's early suggestion was made in a somewhat vague form: the active part of the solute might be a hydrate, but the converse proposition, that a hydrated solute was necessarily an electrolyte, could obviously not be maintained. Some additional factor must come into play in order to produce ionisation. Armstrong, in 1886 (*Proc. Roy. Soc.* 1886, **40**, 268), attributed the increased molecular conductivity of dilute solutions to the gradual resolution of the more or less polymerised molecules of the salt into simple molecules or monads, which when combined with the solvent constituted a "composite electrolyte," to which alone the conductivity of the solution was due; to this he has recently added that "the electrolytically effective monads must be thought of as hydrated in some particularly intimate manner, perhaps as hydroxylated" (*ibid.* 1907, **79**, 595), and the conception has been introduced that a solution may contain isomeric hydrates of different constitution,¹

¹ Compare Werner, *Neuere Anschauungen in der Anorg. Chemie*, Braunschweig, 1905, p. 169.

of which two are active in electrolysis and the third inactive (*ibid.* 1908, 81. p. 86).



This view of the nature of ionisation has the advantage of assigning a perfectly definite meaning to the coefficient of ionisation, but does not lend itself so readily as the theory of electrolytic dissociation to an explanation of the double osmotic pressure of a binary electrolyte such as KCl, the independence of the ionic mobilities, or the additive properties of salt solutions. The tendency in recent years has, therefore, been to amalgamate the two rival theories of solution by means of the hypothesis of hydrated ions—a hypothesis that retains all the advantages of the theory of electrolytic dissociation, but has the merit of recognising also the combination of solute and solvent which forms the essential feature of the hydrate theory of solutions.

THE HYDRATE THEORY OF IONISATION

The hypothesis of hydrated ions has now received somewhat general acceptance, but its significance as an essential factor in the ionisation of aqueous solutions has up to the present received very scanty recognition; there has also been some difference of opinion as to the exact form which the hydrate theory of ionisation should take. Werner, who was one of the first to detect the existence of complex ions and to recognise their importance in electrolytic dissociation, limited the process of hydration to the metallic radical, and considered that the hydrated kation was a compound of fixed composition (usually a hexahydrate) analogous to those which are formed by the addition of ammonia to the metal in the cobaltamine and other similar salts. This view, enunciated in 1893 (*Zeit. Anorg. Chem.* 3, 294), was reiterated with the same limitations in the course of the discussion on "Ionisation" in Section A of the British Association at Leicester in 1907. Armstrong, on the other hand, had laid stress on the association of the solvent with the negative radical.

The form of the theory advocated by the writer, in conjunction with W. R. Bousfield, in a number of recent papers (*Phil. Trans.* 1904, 204, 282; *Trans. Faraday Soc.* 1905, 1, 197; 1907, 3, 123), agrees with that put forward by Abegg (*Zeit. Anorg. Chem.*

1904, **39**, 359) in recognising the combination of the solvent with both anion and kathion, but appears to differ from it in laying greater stress on this property as the one efficient cause of ionisation. The formation of hydrated ions is regarded as the final stage in the progressive hydration of the salt molecule, and has been compared with the biological process of karyokinesis, whereby the excessive growth of a cell results in the formation of two separated individuals each with its own nucleus. The amount of combined water may vary in the case of different ions, and is probably very small indeed in the case of hydrogen and hydroxyl ions; this hypothesis accounts not only for the great mobility of these ions, but also for the low coefficients of ionisation which are frequently observed in the case of acids and bases, but are much less common in salts which contain neither of these radicals; there is no reason, however, for supposing that the quantity of combined water is the sole factor in determining the coefficient of ionisation of a salt. The (average) amount of water in combination with the kathion or anion need not be an exact whole number; indeed, the (average) size of the ion may vary both with the temperature and with the concentration of the solution (Bousfield, *Phil. Trans.* 1906, **206**, 127). It is, however, an essential characteristic of the theory that the solute must be combined with a greater quantity of solvent in the ionised than in the non-ionised condition,¹ since otherwise there would be no force available for producing ionisation, and dilution would favour the formation of the inactive rather than the active form of the solute.

Although primarily designed to explain the behaviour of aqueous solutions, the hydrate theory of ionisation can readily be modified so as to apply to any solvent which possesses the property of forming compounds of greater stability (*i.e.* with greater liberation of energy) when combined with a dissociated than with an undissociated salt. It is also applicable to the case of fused salts; if it is supposed that these also consist of an active and an inactive part, the active part may consist of complexes such as $\overset{+}{K}(KCl)_x$ and $\bar{Cl}(KCl)_y$, and the inactive part of molecules such as KCl or $(KCl)_z$; complexes of the former type may also exist in concentrated aqueous solutions which exhibit "autolytic" conductivity, *i.e.* conductivity which is due to

¹ An opposite conclusion has been arrived at by H. C. Jones and Stine (*Amer. Chem. Journ.* 1908, **39**, 402).

the liquefaction of the salt rather than to any chemical interaction with the solvent.

HYDRATION OF THE IONS DEMANDED BY THE LAW OF MASS-ACTION

It was unfortunate that the chemical aspects of ionisation, which were clearly recognised when the problem of ionisation was first discussed in 1883, were to a large extent overlooked and set on one side when the theory of electrolytic dissociation was developed four years later. The earlier idea of association as a cause of ionisation was regarded as incompatible with the newer theory of dissociation, and for some years the only possibilities that were recognised were that the solution might contain either hydrated molecules *or* anhydrous ions. When at last it was recognised that hydrates and ions might exist in the same solution,¹ the assumption was generally made—in defiance of all the principles of mass-action—that hydrates only existed in concentrated solutions, and were decomposed by further dilution.²

It is a remarkable illustration of the hypnotic influence of Arrhenius's theory that those who adopted it were able to blind themselves so completely to the commonplace requirements of chemical theory as to assume—apparently without any scruples or qualms of conscience—that the effect of adding water was to dehydrate the solute, and that if a sufficiently large excess of water was added the salt was rendered completely anhydrous! Even at the present time it is customary to express the theory of electrolytic dissociation by symbols which represent the ions as anhydrous, and the question of water-combination is treated as of secondary importance.³ Indeed, it is doubtful whether the

¹ "It might well be the case that complex molecular aggregates were capable of existence alongside of dissociated molecules where ions are present. In the case of solutions of sulphuric acid, for example, it is by no means inconceivable that aggregates of several molecules of sulphuric acid (H_2SO_4)_n, or of compounds of acid and water, such as $\text{H}_2\text{SO}_4 \cdot 2\text{H}_2\text{O}$, etc., might exist along with the ions of dissociated sulphuric acid, 2H and SO_4 , or more probably H and HSO_4 ," (Ramsay, *B.A. Report*, Leeds, 1890, 325).

² For an illustration of the prevalence of this opinion see Jones and Getman (*Amer. Chem. Journ.* 1904, 31, 356). The experimental error on which this conclusion was based was subsequently corrected in a German translation of the paper.

³ "The question of hydration, though of great interest, is of secondary importance for the electrolytic dissociation theory" (Senter, *Science Progress*, January 1907, footnote p. 386).

hydrated character of the ions in aqueous electrolytes would even now have secured general recognition but for the fact that it was necessary to assume the existence of complexes of this type in order to account for the apparently anomalous character of the ionic mobilities, and certain related properties of the solutions.

HYDRATION AS RELATED TO IONIC MOBILITY

Having discussed somewhat fully the fundamental problems associated with the application of the hydrate theory to electrolytes, it only remains now to refer briefly to some of the secondary consequences arising from the hydration of ions. They are of interest, not so much on account of their intrinsic importance, as because of the part they have played in convincing those who accepted the theory of electrolytic dissociation in its original form of the necessity of extending their conceptions, even at the cost of sacrificing the extreme simplicity which constituted one of the great attractions of the theory.

Foremost among the properties which have thus afforded evidence of ionic hydration must be placed the mobilities. If, for instance, the alkali metals are compared, the remarkable result is arrived at that, when acted on by equal forces, the metal of highest molecular weight moves most rapidly, and that lithium with a molecular weight twenty times smaller than that of caesium, moves with only half the velocity of the latter metal:

Li	Na	K	Rb	Cs
33'44	43'55	64'67	67'6	68'2

The halogens afford a similar, though less striking, illustration, the bromine atom, with an intermediate atomic weight, being a trifle more mobile than either chlorine or iodine:

Cl	Br	I
65'44	67'63	66'40

In the case of elements so similar in character it is difficult to imagine that there is any wide difference in the structure of the different atoms, and the explanation universally adopted is that the ions are not mere naked atoms, as was at first supposed, but are clothed with a "watery atmosphere"—a happy expression for which we are indebted to Kohlrausch. The quantity of water attached to the ion is assumed to vary in the different members of the series, and the sluggish motion of the lithium ion is attributed to the excessive quantity of water with which

it is encumbered—a view that receives support from the hygroscopic character of the chloride as compared with those of the other alkali metals.

These general views have received an important extension by the introduction of quantitative considerations. Bousfield, in two recent papers (*Proc. Roy. Soc.*, February 10, 1905, compare *Zeit. phys. Chem.* 1905, **53**, 257; *Phil. Trans.* 1906, **206**, 101), has revived the theory (which had generally been discarded on account of the anomalies quoted above) that the velocity with which an ion moves through a solution is governed by Stokes' Law in reference to the motion of a sphere in a viscous medium. According to this view the mobility of an ion is inversely proportional to its radius, and by assuming a suitable value for the density of the combined water it is possible to calculate the number of molecules associated with each equivalent of the ion.

As a further result of the application of Stokes' Law it was found possible to obtain evidence of a shrinkage of the ionic radius as the concentration of the solution was increased. In the most dilute solutions the ionic mobility is probably, as Arrhenius assumed, a fixed quantity which does not vary with the concentration, and the coefficients of ionisation can therefore be calculated accurately from the molecular conductivities. In the case of a few weak acids and bases the simple mass-law equation of Ostwald $\frac{a^2}{1-a} = \frac{k}{v}$ can be used to express the course of the dissociation, but in most cases it is necessary to use a variable index $\frac{a^n}{1-a} = \frac{k}{v}$ as proposed by Rudolphi. It is remarkable that in a large range of compounds the index n is very near to the exact figure $n = \frac{2}{3}$ proposed by van't Hoff, and Bousfield has shown (*Phil. Trans.* **206**, pp. 155-157) that in the case of sodium and potassium chlorides the index approximates more and more closely to this figure as the dilution becomes more extreme, and the risks of variations of ionic mobility are eliminated. The assumption appears, therefore, to be justified that this equation with index $\frac{2}{3}$ represents correctly the course of the dissociation throughout the lower ranges of concentration and that the deviations observed at moderate dilutions are due to changes of ionic mobility as a result of alterations in the viscosity of the medium, or in the dimensions of the ion. The former quantity can be measured without difficulty, and when

this has been done the changes of ionic radius can be deduced. In the case of potassium chloride the relationship

$$r = r_{\infty} (1 + Bk - \frac{3}{2})^{-1}$$

was deduced for the radius of the ion and was shown to hold good up to a concentration of $N/5$, where k = number of molecules of water added to each molecule of salt, B = a constant: the change of radius appears therefore to proceed in a regular manner and according to perfectly definite laws.

Further evidence of variations in the size of the ion (and therefore, incidentally, of the existence of a variable water-atmosphere) has recently been obtained in the case of the hydrogen ion itself. In the case of most of the ions Arrhenius's assumption that the mobility is independent of the concentration holds good for solutions of moderate dilution. The hydrogen ion, however, gave discordant values when the mobility was determined (*a*) from transport values at moderate dilutions and (*b*) from conductivity measurements at extreme dilutions. These discrepancies were traced by Noyes and Kato (*Journ. Amer. Chem. Soc.* 1908, **30**, 318-334) to variations of mobility taking place at dilutions considerably greater than those at which the mobilities of the other ions become constant, and as concordant values were obtained from independent observations with nitric and hydrochloric acid there can be little doubt of their correctness.

Ionic Mobility of Hydrogen at Different Dilutions

Concentration.		Mobility.	
HNO ₃	HCl	HNO ₃	HCl
0.058 \underline{N}	0.051 \underline{N}	350.3	344.2
0.0184	0.017	340.2	340.5
0.0067	0.0056	339.1	341.4
0.0022	0.0021	332.2	331.8
0.0	0.0	324.6	324.0

Collateral evidence in reference to the hydration of the ions has been obtained from a study of electrolysis in mixed solvents, *e.g.* silver nitrate in aqueous methyl alcohol, and of aqueous solutions containing a salt and an inert solute, but the results obtained have not been very decisive, and in any case can only be used to indicate *differences* in the hydration of anion and cation. The method, although of great value in the study of other complex ions, has therefore not proved of equal value in the investigation of hydrates and a passing reference to it must here suffice.

THE TRANSFORMATION OF ELEMENTS.

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PART III

SECTION	PAGE
1. THE PRODUCTION OF HELIUM DURING RADIOACTIVE CHANGES	222
2. RECENT ATTEMPTS AT TRANSMUTATION	226
3. CHEMICAL EVIDENCE OF TRANSFORMATION	236

SECTION I.—THE PRODUCTION OF HELIUM DURING RADIOACTIVE CHANGES

MENTION has been made already of the change of radium emanation into helium, predicted by Rutherford and Soddy, and actually observed by Ramsay and Soddy. The apparatus employed was in great part similar to that shown in fig. 1, Part II. (p. 74). The initial observation, of which an account was published in 1903, was made with the gases evolved from 20 milligrams of pure radium bromide, dissolved in water. The hydrogen and oxygen were removed by contact with a red-hot spiral of copper wire, partially oxidised, and the resulting water vapour by a tube of phosphorus pentoxide. The remaining gas was forced into a small spectrum tube connected with a U-tube. It showed the spectrum of carbon dioxide; but on cooling the U-tube in liquid air most of the carbon dioxide condensed, and the yellow line (D_3) of helium became visible in the spectrum. This was conclusively identified.

A second experiment with 30 milligrams of bromide gave a better result: the carbon dioxide was removed completely, and most of the lines in the helium spectrum were observed. Several further experiments were carried out with similar results.

Attempts were made, which have already been described (p. 76), to measure the volume of the emanation. The capillary tube used for measurement had a very fine platinum electrode fused in at the upper end. As the gas was confined over mercury, the capillary itself could be used as a spectrum tube. In this experiment the emanation was condensed at liquid air

temperature, and the hydrogen and any helium pumped off (see p. 75). After a series of measurements had been made during twenty-eight days, a scarcely visible amount of gas remained. On passing a spark and freezing out the mercury vapour, the helium spectrum was visible.

This experiment proved conclusively that helium is a product of the disintegration of radium emanation. Whether it is produced directly from radium itself can only be ascertained when the relative volumes produced from radium and emanation together, and from emanation alone, have been accurately determined.

The change of radium emanation into helium has been confirmed by numerous observers. Since this is perhaps the most striking of all the transformations, some account is given of their work.

In nearly all cases the spectrum was observed with a direct vision spectroscope. The eye-piece was adjustable by means of a micrometer screw, which could be read to the hundredth part of a millimetre. The spectroscope was either standardised previously by observing a series of known spectra, or else the spectrum of the suspected gas could be observed simultaneously by means of a comparison prism. In the former case a convenient method is to use a tube containing hydrogen, helium, and mercury vapour, which give a number of strong lines throughout the visible spectrum. By plotting the measured screw-readings of these lines against their known wave-lengths, a curve is obtained, from which the wave-length corresponding to any particular measurement can be determined accurately to one or two Ångstrom units.

Curie and Dewar in 1904 confirmed the production of helium in the following manner: About 0.4 gram of radium bromide was placed in a small quartz bulb joined to a quartz tube, and the whole connected with a mercury pump. After evacuation the radium salt was heated till fusion and the whole again evacuated. The quartz bulb and tube were then sealed off. Twenty days later Deslandres examined the spectrum of the gas contained in the tube, using external electrodes. He observed the entire spectrum of helium, and no other lines, although a current was passed through the tube for three hours.

Indrikson carried out an experiment with the gases from

10 milligrams of bromide placed in a vacuum tube, which was then sealed off. He stated that after fourteen days he observed the red, green, blue, and violet lines of the helium spectrum, although the yellow line was absent. Such a conjunction seems unlikely; Himstedt and Meyer consider it probable that the lines observed belonged to the secondary spectrum of hydrogen.

These physicists carried out in 1904-5 a series of most exhaustive tests; the results of their work supplied any final proof, if that were required. In their first experiment 50 milligrams of radium bromide were placed in a U-shaped glass tube, to which was joined a vacuum tube containing aluminium electrodes. The tubes were joined to a pump, and the whole apparatus completely evacuated during seventy-two hours. The tubes were then sealed off. Initially only the spectrum of hydrogen and carbon monoxide were visible. After fourteen months the yellow and green helium lines were seen, but could not be photographed. Three months later most of the strong helium lines were visible. The tubes employed in this, and in most of Himstedt and Meyer's experiments, were comparatively large, which probably accounts for the length of time before sufficient helium had accumulated to give a visible spectrum.

The authors considered it unlikely that the helium could come from the electrodes, but carried out two experiments to test this point specifically. Twenty-five milligrams of the bromide were placed in a small U-tube, to which was joined a small spectrum tube. The whole was connected to a pump, evacuated, and washed out with hydrogen ten times, and during each operation the current was passed to remove completely any helium from the electrodes. Finally, the tubes were exhausted completely and sealed off. After three months the helium spectrum was definitely identified.

The remaining 25 milligrams of bromide were converted into sulphate, and this dried and ignited. It was then placed in a tube of quartz, to which a small electrodeless tube was joined with marine glue. The tubes were connected to a pump, evacuated, and rinsed with hydrogen as usual, and finally sealed off. A Tesla coil was used to light up the spectrum tube. After three weeks the four principal helium lines were observed, and two weeks later they were identified conclusively.

Later the same workers carried out a series of negative tests; in all cases the results were satisfactory.

Giesel performed somewhat similar experiments, and also confirmed the production of helium.

In view of these numerous experiments by different workers, all leading to the same result, the change of radium through its emanation into helium must be regarded as finally proved. It is of interest to see what other radioactive substances produce helium.

Debierne in 1905 repeated Ramsay and Soddy's work with about 10 milligrams of radium bromide. His methods were very similar, and he obtained the same results in numerous experiments carried out during a period of six months. He then substituted very active actinium salts for the radium bromide. A solution of actinium chloride disengaged hydrogen and oxygen in quantity comparable as regards activity with that from a radium salt solution. Treating the gases in the usual way, he succeeded in identifying the helium spectrum visually and photographically. After specially purifying his solution from radium, he still obtained the same results; helium was also observed in the gas produced by heating some dry actinium fluoride. Debierne states that the quantity of helium formed is in all cases comparable with that derived from radium of the same degree of activity. He checked his results by carrying out blank experiments with other gases, especially electrolytic gas; helium was never observed.

In 1907, Giesel, using the methods he had employed for radium salts with a strongly active actinium preparation, succeeded in identifying the D_3 line of helium.

Ramsay has obtained some indications of the formation of helium from thorium solutions, but this cannot yet be regarded as proved. In this connection Strutt's work on thorium minerals, which will be dealt with in the third section, is of great importance.

Greinacher and Kernbaum have carried out observations during eleven months with polonium (radium F) preparations enclosed in evacuated spectrum tubes of small capacity. No trace of helium was observed.

Hofmann and Wölfl state that when radio-lead, or radium D sulphate is placed in a completely exhausted capillary tube, after several months the yellow, green, and blue helium lines become visible, although all attempts with radium E and F have given negative results.

It had been stated in Part I. that Rutherford measured the mass of the α -particle carrying unit charge of electricity; it is about twice that of hydrogen. He concluded that the α -particle consisted either of a half-atom of helium, carrying one electric charge, or a whole atom carrying a double charge. His recent work shows that there is strong evidence for regarding the latter assumption as established. In view of the large number of radioactive substances emitting α -particles it is interesting to note that the only cases in which the production of helium can be regarded as proved are cases in which emanations are present. In this connection its possible production from radio-lead certainly demands careful re-investigation.

The quantity of helium produced from radium bromide was measured roughly by Ramsay and Soddy. It is of an order which, when considered in connection with the total amount of radio-active changes taking place, lends support to the suggestion that all the helium in the earth's atmosphere has been produced by such transformations. The questions must at once arise: are these transformations limited to the production of helium? can no other gases of the argon series be produced thus?

SECTION II.—RECENT ATTEMPTS AT TRANSMUTATION

The alchemistic doctrine of transmutation was accepted by most thinkers until the beginning of the sixteenth century. The modern definition of an element is due to Boyle, and scarcely can be said to have received wide acceptance by the end of that century. His predecessors put forward some form or other of the *elements* of the ancients, of which the Aristotelian system—earth, air, fire, and water—is typical. Such elements were in reality *properties*, which each substance possessed in different degrees. With such conceptions a belief in transmutation might easily arise, since if two *substances* consisted of different proportions of these *elements*, they should be capable of transformation into one another by altering the proportions.

The belief in transmutation arose at a very early date; attempts to transmute base metals into gold or silver date to the Egyptian era. (The name *χημεία*, given to this art, is possibly derived from *chémi*, an old name for Egypt.) Superficial observations of a nature easy to comprehend led to these attempts. Thus iron utensils left in copper mines were coated with a

deposit of copper from the accumulated waters; copper could be changed into white or yellow alloys by admixture with various earths. The transformations into copper, and into silver or gold, were apparent.

The early history of chemistry is largely the history of such attempts at transmutation, with various discoveries incident thereon. As chemical knowledge grew, the evidence of these changes became more and more capable of disproof, and belief in them was only upheld by deception and self-deception.

With the acceptance of Boyle's definition of an element, the belief in transmutation in great part vanished. Dalton's hypothesis of definite indivisible atoms peculiar to each element made it appear still less likely. Not until the development of the subject of radio-activity, and in particular till Ramsay and Soddy showed that radium is transformed into helium, could any support be given to the belief.

It is necessary at this stage to distinguish between the terms "transformation" and "transmutation." The element-changes so far dealt with, which take place in nature under normal conditions, are examples of *transformation*. *Transmutation* contains the idea of active human intervention, and it is convenient to ascribe it to changes so brought about where they differ from those which would have occurred without such intervention.

The old and new ideas of transmutation differ greatly. The alchemist sought to transmute large quantities of material, and made the attempt for personal gain. The modern chemist knows that such transmutations, if possible, can only be carried out in extremely small quantity, by the expenditure in some form or other of a relatively enormous amount of energy. The product of transmutation will be probably of less value than the original element. Thus the idea of gain is eliminated, and the study of these changes can have no commercial application.

As an example of these ideas the recently published experiments of Th. Gross may be cited. An alternating current of high voltage was passed for several hours through a platinum crucible containing fused potassium carbonate at a red heat; potassium nitrate was added at intervals. The electrodes consisted of stout platinum wire, and it was found that an appreciable quantity of platinum dissolved, both from the electrodes and from the crucible. The melt, when examined, gave an

insoluble substance, resolvable into two parts, one resembling platinum, but differing considerably in its chemical behaviour. Gross assumes that platinum is decomposed into a new metal; to regard this as a "modification" is an unjustifiable extension of that term. His results await confirmation.

The most convenient source of a relatively enormous amount of energy, which can be set free locally, and allowed to act on the molecules and even the atoms of different substances, is unquestionably radium emanation. The enormous store of energy liberated as each particle disintegrates has been mentioned. When this is liberated in water, the water is decomposed; an account of this, and of the effect on various gases, has been given in Part II. (p. 82). The changes there discussed are molecular. It is now necessary to consider what proofs have been put forward for atomic changes produced by this agency.

From the fact that radium emanation decomposes water into hydrogen and oxygen, Sir William Ramsay was led to try its effect on a solution of copper sulphate, with the idea that there would result a deposit of copper equivalent to the hydrogen. That such an idea was justifiable is proved by his results with silver solutions, as yet uncompleted, in which a deposit of silver is actually formed, while oxygen is liberated, along with only a very small quantity of hydrogen. In the case of the copper solution, however, no copper was deposited, but on analysing the solution a trace of lithium was observed spectroscopically. Such a result was quite unlooked for. The copper sulphate was prepared from the ordinary pure salt, recrystallised four times. Laboratory copper sulphate invariably contains lithium, but it has been found that after such a series of crystallisations, the lithium is entirely eliminated.

The experiment was repeated several times. On the second occasion copper sulphate was prepared from pure copper electrolytically deposited on platinum, and "contact" sulphuric acid. The copper sulphide from this experiment was converted into acetylides, and that into nitrate; this was employed in a third experiment.

In all cases a trace of lithium was found after treatment with emanation. This lithium might result in four ways—(a) as impurity in the copper salt, (b) from the glass bulb in which the experiment was carried out, set free either by action of the emanation or of the salt solution, (c) as impurity in the reagents

used in analysis or from the vessels employed, or (*d*) as a transmutation product from one of the elements present, in which case, since the same results were obtained with both nitrate and sulphate, the probable parent substance would be copper.

To test these possibilities Ramsay and Cameron carried out a series of three experiments as carefully as possible.

(1) Pure copper nitrate solution was treated with emanation in a glass bulb. After treatment the solution was analysed. Lithium was present.

(2) A roughly equal quantity of the same solution was placed in a bulb of the same glass, sealed up, and allowed to stand along with the first. No lithium was found.

(3) Pure water was placed in a similar glass bulb, and treated with emanation. Afterwards it was evaporated to dryness and the residue tested. No lithium was found.

The further results obtained in these experiments will be discussed presently.

Since no lithium was found in the second experiment (*a*) was ruled out. The results of the second and third experiments together negative (*b*). Since the "treated" and "untreated" solutions were analysed in identically the same way, hypothesis (*c*) seems extremely unlikely, so that an actual transmutation would seem to have taken place.

Some account of the methods used in these and later experiments will now be given.

(*a*) *Preparation of copper nitrate*.—Pure laboratory copper sulphate was dissolved in extremely pure water (the water used throughout these experiments was of such degree of purity that 20 c.c. gave a totally unweighable residue); the solution was electrolysed and the copper deposited on a rotating cathode of smooth platinum. After washing with pure water about half the copper was dissolved in a few cubic centimetres of redistilled nitric acid of such degree of purity that 50 c.c. gave a residue of less than 0.4 milligram. The solution was evaporated to dryness on the water-bath, and heated for some time to expel nitric acid. It was dissolved in pure water and filtered into two glass bulbs, newly made from the same piece of tubing. The first was treated with several accumulations of emanation. It was finally sealed off and the emanation allowed to decay. The second was exhausted and sealed.

(*b*) *Method of treatment with emanation*.—The original plan has

been modified slightly to allow the substitution of silica for glass bulbs. It will be described in its present form. The apparatus is shown in fig. 1.¹

The bulb 1 is, if of silica, joined to the rest of the apparatus by means of a ground silica-glass joint at H; if of glass, it is directly joined at H. The bulb is half filled with the solution, which is to undergo treatment, by means of a second side tube, not shown in the figure; this is then sealed off. The whole

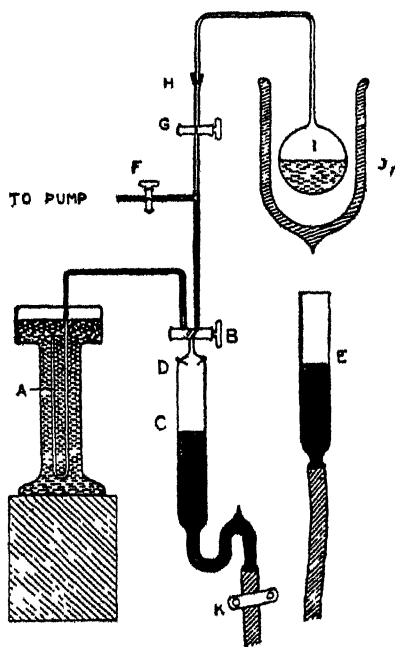


FIG. 1.

apparatus is evacuated. The emanation, and hydrogen and oxygen, obtained from radium bromide solution in the manner already explained (p. 73), are transferred as usual through the inverted siphon A into the burette C, and there exploded; the excess of hydrogen and the emanation are admitted to the bulb. This is then cooled to -185° by means of liquid air, and the hydrogen pumped off as completely as possible through the tap F. Mercury is allowed to rise just above the ground joint H, and acts as a seal, preventing any leakage. In most cases more

¹ Figs. 1 and 2 are reproduced from the *Transactions of the Chemical Society* by kind permission of the secretaries.

than one *dose* of emanation is added. The additions are made in a similar manner, at intervals of three or four days. In each case the gases formed since the last addition are previously pumped off as completely as possible and stored. The total quantity of gas is analysed at the end of the experiment (see below, (d)), and the solution also analysed.

(c) *Method of analysis of the solutions.*—In each case the copper nitrate solution was transferred to a platinum crucible, and saturated with sulphuretted hydrogen passed through a platinum tube. The sulphide precipitate was separated by centrifugalisation in a silica tube, and the filtrate evaporated to dryness and ignited. The residue was treated with a few drops of pure water, and the solution, which should contain all the alkali salts, was evaporated, ignited, weighed, and examined spectroscopically. The whole of the analysis was carried out in either silica or platinum vessels.

In both experiments calcium and sodium (strong) were present. Lithium was present in the "treated" case only. The weight of the alkali residue was different in the two cases: that from the "treated" solution was distinctly larger. This would lead to the assumption that sodium is also a transmutation product, but further evidence is necessary before stress can be laid on these weight relations. The treatment of the pure water in the third experiment has been indicated already.

(d) *Method of analysis of the gaseous products.*—The methods employed for determining the amounts of the ordinary constituents of a gas mixture were those devised by Ramsay himself. They are applicable to quantities as small as one or two cubic centimetres, the results in such cases being accurate to within 1 per cent. The actual measurements are carried out in a gas burette provided with a series of points sealed to the glass, similar to those shown in fig. 3, Part II. p. 83, and the measurements are made in a similar manner, the pressure exerted by the gas being measured, from which its volume can be calculated. (The calibrated gas volume is that from the tap to a mercury-point surface.)

In the gas-mixtures dealt with, the constituents are, as a rule, only hydrogen, oxygen, the oxides of carbon, nitrogen, and inert gases. The total volume is first measured. The gas is then forced back through the inverted siphon into a test-tube and allowed to remain for a few minutes in contact with moist

potash. After again remeasuring a spark is passed; if necessary oxygen is added, and the mixture again exploded. The gas is treated with potash for the second time; the contraction in this case is due to carbon monoxide. From these series of measurements the amounts of hydrogen, oxygen, and the oxides of carbon are easily calculated. Finally, the gas is transferred to a second test-tube containing a pellet of phosphorus, and the excess of oxygen thus removed. The residue consists of nitrogen and inert gases.

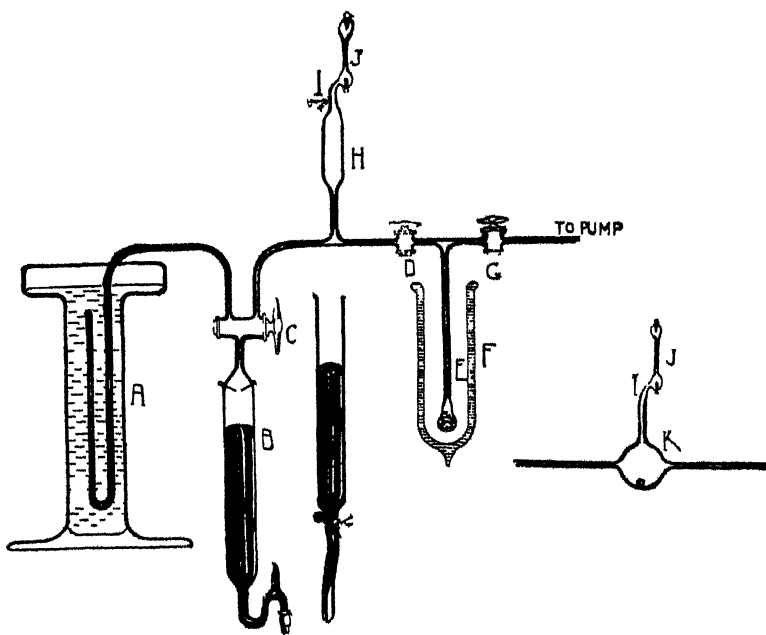


FIG. 2.

In examining these, advantage is taken of the fact, discovered by Dewar, that charcoal cooled to the temperature of liquid air absorbs nitrogen and the heavier inert gases. Neon and helium are not absorbed. It is thus possible to separate them from nitrogen and argon. The separation is carried out in the apparatus shown in fig. 2. The gases are taken through the inverted siphon into the burette B, the rest of the apparatus is evacuated through the tap G connected to a mercury pump. During this operation the bulb E containing charcoal is heated at 230° in quinoline vapour to free the charcoal from adsorbed gases, and at the same time a current is passed through the spectrum

tube j to remove hydrogen and any other gas from the electrodes. g is then closed. The stopcock c is opened and the gas allowed to enter the apparatus; to prevent leakage mercury is raised above the stopcock, which is then closed. e is cooled in liquid air, and the charcoal adsorbs any nitrogen and argon (also krypton and xenon) present. The tube h is surrounded by a paper cup, moistened with water; on filling this with liquid air a vessel of ice is produced, in which the liquid air evaporates comparatively slowly. The object of this precaution is to prevent loss of helium and neon through their contraction to small volume in the cold region e. d is then closed and mercury forced up through c to the point i. The spectrum can then be examined, and, if desirable, the spectrum tube sealed off.

The stopcock g is next opened, the charcoal again heated to 230° , and the gas pumped off and collected in a test-tube over mercury. Oxygen is added to it, and sparks from an induction coil are passed through the mixture for some hours; a small quantity of potassium hydroxide absorbs all the oxides of nitrogen formed. The residue, oxygen and inert gases (minus helium and neon) are then admitted to an apparatus similar to that just described, but without the charcoal bulb, containing instead phosphorus in a bulb k to absorb the oxygen. After this has taken place, mercury forces any gas that remains into the small spectrum tube, and its spectrum can be examined.

The three experiments of which mention has been made gave the following results. From the "untreated" copper nitrate no gas could be pumped off, as of course was expected. The "treated" water gave hydrogen, oxygen, traces of the oxides of carbon, and a small quantity of nitrogen. The experiment with copper nitrate gave a small quantity of nitric oxide (observed by the formation of red fumes during admixture of the portions of gas pumped off before each addition of emanation). There were also present hydrogen, oxygen, and small quantities of carbon dioxide and nitrogen.

The inert gas from the experiment with pure water consisted almost entirely of neon. Initially the spectrum tube gave the blazing red colour of neon, tinged slightly heliotrope owing to the presence of a small quantity of helium. Only the red, yellow, and green lines of helium were visible; the blue and violet lines were not observed.

In the gases from the copper solution no neon or helium

was observed, but argon only. The trace of nitrogen present in this case would perhaps account for the argon, but the absence of helium, which is not easily masked by argon, points to a possibility that some part at least of the argon was derived from the emanation.

Work on similar lines is being carried out in silica bulbs, in order specially to observe whether there is any constant difference of weight in the alkali residues obtained from "treated" and "untreated" solutions of copper salts and those of similar elements. Incidentally, such an experiment with pure water gave an unweighable residue, which did not contain lithium, showing that the silica is unaffected, and that the method is therefore applicable. This experiment also confirmed the production of neon. In this case the neon and helium spectra were comparable in intensity, the latter being somewhat stronger. The spectrum was photographed, and some twenty neon lines conclusively identified.

In considering these results it is necessary to bear in mind two important considerations. Soddy and Mackenzie find that traces of helium and neon can always be obtained from aluminium electrodes which have been used previously with those gases, and even with fresh electrodes. Strutt has found that admixture of helium with one cubic centimetre of air, and with care, with one-tenth of that amount, renders the neon spectrum visible, after separation of other gases with cooled charcoal. The answer to objections based on these observations seems to lie in the fact that in the experiments described the amounts of neon observed were not mere traces, but were in one case much greater than the helium present, and in the second of the same order. Again, other experiments with the same spectrum tubes did not result in the production of neon, so that it scarcely can come from the electrodes, although these were of aluminium. There is therefore strong ground for belief that neon is a disintegration product of radium emanation under certain conditions, and since these are brought about by the intervention of human agency, it can be regarded as a case of transmutation.

The cases for argon, and for the copper-lithium change, are by no means so strong. One-tenth of a cubic centimetre of air contains sufficient argon to be detected with ease spectroscopically, so that slight leakage would always result in its

presence. Further work has shown that lithium exists very widely in minute traces, and that it is very difficult to free salt solutions from its presence completely. All that can be stated at present is that in the actual experiments just described lithium was certainly detected in the "treated" solution, and was not detected in an "untreated" solution otherwise identical.

In August 1908 Mme. Curie and Mlle. Gleditsch published an account of experiments devised especially to test the supposed copper-lithium transmutation. They found that when pure water was allowed to stand in contact with glass, the dissolved salts contained lithium. On this account the whole of their work was carried out in platinum vessels. They used a solution of copper sulphate fractionally recrystallised a large number of times to free it from lithium. The quantities of copper sulphate and of radium emanation employed were comparable to those used in Ramsay and Cameron's experiments; no lithium was detected in the solution after treatment, moreover the weights of the alkali residues from "treated" and "untreated" solutions did not differ distinctly. The authors conclude that the transmutation of copper into lithium and sodium cannot be held as established. It is evident that much further work is necessary before the matter can be decided definitely.

It will be observed that the changes of which indication has been found follow the periodic system strictly. The emanation, the highest member of the argon series, breaks down into helium, and under certain conditions into neon, and perhaps argon. Copper, when subjected to the same forces, possibly gives lithium, and also sodium. In this connection it is desirable to bear in mind that the spectroscope, on which this evidence is based, allows the detection of smaller amounts of matter than any other known method except the measurement of radioactivity. Ramsay and Cameron, by comparison with known mixtures of lithium and sodium, estimated the amount of lithium obtained from about a gram of copper nitrate as 0.00017 milligram. Mlle. Gleditsch, in work which will be referred to in the next section, has estimated that it is possible to detect spectroscopically one part of lithium in 10,000 of sodium.

With a sufficiently small spectrum tube it is easy to detect spectroscopically amounts of the rare gases of the order of 0.1 cubic millimetre. (Perhaps considerably less can be detected.) Such a volume in the case of neon weighs 0.0001

milligram, an order of magnitude corresponding to that in the case of lithium.

The measurement of minute quantities of gases probably ranks next in accuracy. With great care, using small apparatus, the presence of 0.01 cubic centimetre of, say, carbon dioxide can be detected with certainty. This volume weighs 0.02 milligram. Using a micro-balance, quantities of not more than one or two milligrams can be weighed with an accuracy of one part in a thousand. But such quantities are too small for experimental work of the nature described. Where changes of weight are concerned, in which an ordinary balance must be employed, the utmost degree of accuracy attainable is 0.02 milligram. Where the substances or solutions must be put through several chemical operations the degree of accuracy is considerably lessened.

The transformations of elements were detected first by radioactive measurements; the production of helium was proved by spectroscopic evidence, on which also rests the case for the transmutations dealt with in this section. The detection of transmutation is therefore most likely in cases where the spectrum of the product is easily recognisable. In order to produce a measurable amount of a gas, or a definitely recognisable change of weight, a relatively large amount of transmutation must take place.

Transformations occurring in nature through long periods of time would lead to the accumulation of much larger amounts of products, and these should therefore be much more easily detected.

SECTION III.—CHEMICAL EVIDENCES OF TRANSFORMATION

It has been pointed out in Part I. (vol. ii. p. 531) that when one radioactive element disintegrates into a second, and the life of the latter is shorter than that of the first, then ultimately a state of equilibrium is reached, in which the quantity of the second element disintegrating in any given time is equal to the amount formed in that time. Under such conditions, provided the life of the parent element is great in comparison, so that its amount is practically constant, then the amounts of the two in any mineral containing them must be in a definite ratio. This holds true even if intermediate products occur, provided that their life is sufficiently short to permit the state of equilibrium

to be reached. Uranium and radium give a good example of such a fixed ratio.

Again, the ultimate disintegration product—of course a non-radioactive element—will accumulate in ever-increasing amount as the parent substance, and the mineral containing it, grow older. Consequently the amount of this product in any mineral must depend on the age of the mineral. Lead is supposed to be the ultimate disintegration product of the uranium-radium series.

The evidence put forward to support these statements will now be considered.

The method employed to estimate the amount of radium in a mineral has been gradually evolved from those used by Boltwood and Strutt. A known weight of the mineral is decomposed with acids, and the solution boiled to expel accumulated emanation completely. It is then kept in a closed vessel, and the emanation allowed to accumulate for a known period. The solution is again boiled, the emanation removed, and measured quantitatively. From the amount of emanation formed in a given time the amount of radium can be calculated. If thorium or actinium be present in the mineral, then its emanation will also be present. By observing the initial rate of decay of the mixed emanations, the relative amounts of the thorium and radium emanations can be determined. Actinium emanation decays so rapidly that it can scarcely be detected in such a mixture unless present in relatively large amount. The amounts of thorium and uranium present in minerals are determined by the usual methods of chemical analysis, though Strutt has found it convenient and accurate to estimate the thorium from the amount of its emanation.

The following figures are taken from a much larger table published by Boltwood in 1905; the emanation equivalent is given in arbitrary units:

Mineral.	Locality.	Emanation equivalent of radium in 1 gram of mineral.	Weight in grams of uranium in 1 gram of mineral.	Ra. Ur.
Uraninite	N. Carolina	170.0	0.7465	228
"	Joachimsthal	139.6	0.6174	226
"	Saxony	115.6	0.5064	228
Uranoplane	N. Carolina	113.5	0.4984	228
Carnotite	Colorado	49.7	0.2261	220
Orangite	Norway	23.1	0.1034	223
Thorite	"	16.6	0.0754	220
Monazite	Brazil	0.76	0.0034	223

Boltwood points out that since the uranium-radium ratio is constant within the limit of experimental error, the inevitable and only possible conclusion is that uranium is the parent of radium. He adds also that, since the uranium-radium ratio in thorite, which contains about 50 per cent. of thorium, is the same as in minerals, such as uranoplane and carnotite, containing a mere trace of thorium, thorium cannot possibly be regarded as the parent of radium.

He tested thirteen such minerals for lead, and found an appreciable quantity in all, except in a specimen of uranoplane, which contains a mere trace, and which is the youngest geologically. Lead minerals do not exist side by side with most of the minerals tested, so that this possible source of lead is excluded.

In a later paper, Boltwood examined previously published analyses of uranium minerals; the ratio of lead to uranium in forty-three minerals varies from 0.04 to 0.25. Minerals from the same locality show very good agreement. He quotes Barrell's opinion that the variations of the ratio for minerals from different localities "are not contradictory to the order of the ages attributed by geologists to the formations in which the different minerals occur." Assuming that the helium in minerals is formed by disintegration (this statement will be discussed immediately), and according to the equation—

$$\text{Uranium (238.5)} = \text{lead (206.9)} + \text{helium (31.6)}$$

(this assumes that helium is identical with the α -particle, and that eight α -particles are liberated in the uranium disintegration)—he finds that the actual amount of helium in minerals never exceeds the theoretical quantity, and varies from 6—100 per cent. of that quantity. The denser minerals usually retain the larger proportion.

Strutt has analysed twenty minerals for radium and uranium, and his figures, although showing greater variation than those of Boltwood, yet point conclusively to the same result.

McCoy and Ross found that the total radioactivity of minerals free from thorium, but containing actinium, bears a constant ratio to the amount of uranium present. This led them to the conclusion that actinium is also a product of uranium. As it has been shown (see Part I. vol. ii. p. 544) that actinium is not in the uranium-radium series, this would point to the existence of an entirely different uranium-actinium series of disintegrations.

Since he finds that all thorium minerals contain readily detectable quantities of uranium, while some uranium minerals are practically free from thorium, Strutt concludes that uranium is derived from thorium. Boltwood points out that in the former case the minerals were much older than in the latter; he thinks the evidence can be interpreted on the assumption that thorium is a disintegration product of uranium having a longer life.

There is thus at least a possibility, so far as present evidence goes, that uranium is the parent of all the radioactive elements.

Since Ramsay discovered helium in the mineral cleveite, in which it exists in comparatively large amount, and since Lord Rayleigh proved its existence in the Bath waters, many workers have turned their attention to the existence of the rare gases in minerals and springs. Since Ramsay and Soddy showed that helium is a disintegration product of radium, investigators have tried to discover some relation between the amounts of radioactivity and of rare gases present. Strutt and Moureu have paid special attention to this problem.

Strutt has made an exhaustive study of the composition of radioactive minerals, especially to ascertain the relative proportions of uranium-radium, thorium, and helium present. In his initial experiments he heated the mineral with strong sulphuric acid to obtain its gaseous constituents. These were analysed by methods similar to those described in section 2.

It has been mentioned already that Boltwood has considered the presence of helium in minerals as derived from uranium, neglecting the possible production from thorium. Strutt strongly disputes this view, and his results bear out his opinion. The following figures are taken from a paper which he published in 1905 :

Mineral.	Locality.	Helium c.c. per kilogram.	ThO ₂ per cent.	Radium as radium bromide, millionths per cent.
Thorianite	Ceylon	8.9	77.0	30.4
Monazite	Norway	2.41	1.21	0.275
Samarskite	N. Carolina	1.50	1.46	22.5
Monazite	Fahlun	1.40	0.80	0.323
Aeschynite	Hitteroe	1.09	1.26	24.0
"	Urals	0.98	8.18	9.90
Orangite	Brevig	0.11	48.5	2.82
Pitchblende	Joachimsthal	0.107	—	153.0
"	Cornwall	0.10	—	48.5
Cupro-uranite	"	0.10	—	120.0
Microlite	Virginia	0.05	—	3.70

Strutt points out that in no case does any mineral contain more than a trace of helium unless thorium is also present. When much helium is present, thorium is present in quantity; but the converse is not true, as is shown by the case of orangite.

Later he quotes the case of the mineral fluorite, from Ivigut, Greenland, which yields 27 c.c. of helium per kilogram, contains scarcely any radium, but is rich in thorium. He concludes that the production of helium from thorium must be considered to be established.

Note must be made in passing of the peculiar fact that pitchblende and cupro-uranite, two of the most strongly radioactive minerals known, yield only minute traces of helium.

In a paper published in the present year Strutt tabulates his results for a very large number of minerals, which were examined in order to observe whether (*a*) the degree of radioactivity is sufficient to account for the quantity of helium present, (*b*) argon and neon are present in definite amount.

He finds that helium is present in almost all the minerals of the earth's crust, and is roughly proportional to the traces of uranium and radium they contain. When there is much more helium than would be expected from the amounts of uranium and radium present, it can always be connected with the presence of thorium, with one exception. There is no evidence that helium is produced from ordinary elements.

The mineral beryl contains abundance of helium, but is only very slightly radioactive. The helium cannot be connected with any known constituent of beryl.

With regard to argon, its presence in small quantities is probably due to traces of atmospheric air leaking into the apparatus. Nevertheless it is certainly present in igneous rocks, and probably in siliceous minerals generally. No definite conclusion is reached with regard to the presence of neon. It has been observed in several cases; but Strutt states that an admixture of one cubic centimetre of air makes the neon spectrum visible, and that it may even be seen with one-tenth of a cubic centimetre.

The general results derived from the examination of the gases from springs lead to the conclusion that they are all more or less radioactive, and all contain traces of the rare gases. This is only to be expected in the light of Strutt's results with minerals, through some of which the spring waters must percolate. •

Coming to particular examples, Rayleigh, as has already been stated, detected helium in the gases from the Bath waters, and the amount is such that Dewar was able to employ them as a source of helium.

Moureu has examined the gases from over forty different hot springs throughout France. In all cases rare gases were present in quantities varying from 0.005—6.35 per cent. Of these argon was detected spectroscopically in forty-three cases, helium in thirty-nine. Usually the quantities were of the same order of magnitude. No separation of the two was attempted. Moureu states that his figures agree with the radioactivity of the springs as determined by Curie and Laborde. The larger part of the gases always consisted of nitrogen and carbon dioxide; oxygen was only present in small amount. The rare gases were proportional to the nitrogen, inversely to the carbon dioxide. It is hence at least possible that the argon detected by Moureu had, like the nitrogen, originally an atmospheric origin.

In later work Moureu has shown that traces of neon are usually found. Since his methods were similar to those used by Strutt, the remarks of the latter must be borne in mind *re* the possibility of air-leakage accounting for small traces of neon. Incidentally, Moureu and Biquard estimate that a spring at Bourbon-Lancy produces over ten thousand litres of helium annually, although this only amounts to 1.84 per cent. of the total gas evolved. So that there is some ground for believing that at any rate all the helium in the atmosphere is ultimately derived from radioactive changes.

Cady and McFarland have detected helium in forty samples of natural gas from widely separated parts of America. They observed neon in one instance.

In view of the experiments of Ramsay and Cameron on the production of lithium and other alkali metals from copper, McCoy and Mlle. Gleditsch have determined the amounts of copper and lithium in several radioactive minerals.

McCoy found that four uranium-radium minerals contained both copper and lithium in easily recognisable amounts, while a fifth, a sample of gummite, contained lithium, but no copper. He considered that these results agreed with the possibility of a transformation from copper to lithium.

Mlle. Gleditsch has examined a number of radioactive minerals from different sources. They all contain lithium, but

in quantity which can only be determined spectroscopically. She finds that there exists no simple relation between the copper and lithium present in such minerals, whilst the mineral which contains the largest amount of lithium, a sample of carnotite, is one of the least radioactive. She concludes that whilst the results do not contradict Ramsay's theory, neither do they support it.

Now that it has been proved that one element can be transformed into another, it is inevitable that attempts should be made to explain these facts, and at the same time to suggest a reasonable theory of element-synthesis.

Prout's hypothesis has been revived in new guise. At no time can it be said to have altogether lost support. As originally put forward, it was easily proved untenable, with the advent of exact atomic weight determinations. Nevertheless, the periodic system, and latterly spectroscopic evidence, pointed ever more strongly to some underlying relationship between all the elements. And the development of radioactivity once more concentrated attention on these ideas.

Lockyer, considering the spectra of nebulae, of which the degree of complexity increases as the nebula gets less simple, considers that this points to a gradual building up of elements from the simplest forms.

Perhaps the most ingenious of recent theories of element-synthesis is that put forward by A. C. and A. E. Jessup, which starts with the same idea. From spectroscopic evidence they conclude that matter in the nebula stage consists of four elements, termed *protoms*; of these, two only, hydrogen and helium, are known. These protons are formed from primary corpuscles—the original form of matter—by aggregation into stable integral systems. Other atoms are formed by a process of condensation of corpuscles round the proton-groups, the various atoms representing groups of maximum stability in a gradual and continual process. The authors state that the formation of successive elements is attended by the escape of a large quantity of energy; the systems are steadily progressing from higher to lower energy content. "The whole process of evolution is entirely governed by the possibility of energy leaving the system." They suggest instability as the reason of disintegration, and this is undoubtedly true. The fact, however,

that radioactive disintegration releases an enormous amount of energy (p. 79) seems fatal to their argument just quoted.

It is highly probable that ultimately some theory of evolution and devolution of elements will be put forward, which will approximate to the truth, especially as our knowledge of the facts is increasing rapidly. It is just as certain that no such theory has yet made its appearance.

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THE CLASSIFICATION OF THE PLUTONIC ROCKS

By F. H. HATCH

IN the quantitative classification of igneous rocks devised by Messrs. Whitman Cross, Iddings, Pirsson, and H. S. Washington,¹ the hitherto existing nomenclature of rock-types is entirely discarded, and a new nomenclature introduced which is based on purely chemical considerations without regard to mode of origin. Consequently plutonic, volcanic, and differentiated dyke rocks, if they have similar chemical compositions, find a place in the same divisions, no matter how dissimilar they may be in physical character.

I desire in this paper to show that it is not necessary to throw over the existing rock nomenclature, nor to disregard mode of origin, in basing a natural system of classification on chemical considerations. Apart from mode of occurrence, the difference in internal structure or texture, between a *eugranitic* plutonic rock, that has cooled slowly as a deep-seated mass under pressure and in the presence of occluded water, and a porphyritic or semi-vitreous volcanic rock, that has been erupted at the surface and has cooled quickly, is so great that a system can scarcely be termed natural that places two such different rocks in the same category. In a natural classification, therefore, the plutonic and the volcanic rocks must be recognised as distant classes. It is not necessary for my purpose to discuss here whether a third division for the hypabyssal rocks, to use Brögger's² convenient term for Rosenbusch's *Ganggesteine*, is equally justified or not.

Accepting, then, the plutonic rocks, as a primary subdivision of the igneous rocks, we can apply to them the principles of a chemical classification. The best results are

¹ *Quantitative Classification of Igneous Rocks, based on Chemical and Mineralogical Characters, with a Systematic Nomenclature*, 8vo, pp. 286, Chicago, 1903. For a critical review of this, see A. Harker, *Geol. Mag.* December 4, vol. x. 1903, pp. 173-8.

² *Die Gesteine der Grorudit-Tinguait-Reihe*, Vidensk. Skrifter Math-naturv. Klasse. Christiania, 1894, p. 123.

obtained by arranging them in the order of their silica contents. If with this arrangement, a division into series according to the alkali and calc-alkali contents, as shown by the constituent feldspars and feldspathoids, be combined, a serviceable system of rock classification is obtained.

Arranged in the order of their silica contents, the plutonic rocks can be divided into three groups :

- (1) *Acid*, with silica contents above 66 per cent. ;
- (2) *Intermediate*, with silica contents between 66 and 52 per cent. ; and
- (3) *Basic*, with silica contents below 52.

These limits are, of course, quite arbitrary, but it will be seen later that they permit a convenient separation in accordance with existing records of rock types. At the extreme basic end there is a small assemblage of rocks which contain very little or no feldspar. These rocks (the picrites, peridotites, hornblendites, and pyroxenites) consist of ferromagnesian minerals and iron-ores. They form a sub-group generally known as the *Ultra-basic* rocks, which need not be considered here in the general scheme of classification.

The division according to alkali and calc-alkali contents is accomplished by means of the feldspars and feldspathoids, the variation in which facilitates the formation of a number of series, each series being characterised by its dominant feldspar or feldspathoid and each comprising acid, intermediate, and basic rocks. Thus, a series can be formed which is characterised by the predominance of the lime-bearing feldspars (oligoclase to anorthite). This may be conveniently termed the *calc-alkali series*, and comprises rocks ranging from granodiorite through tonalite and diorite to gabbro, according to the percentage of silica. In contrast with this is the *alkali series*, in which there is a dominant alkali-feldspar (orthoclase, microperthite, soda-orthoclase, microcline, anorthoclase, albite, etc.), or by a feldspathoid (nepheline, sodalite, etc.). Occupying a middle position between the calc-alkali and the alkali series is the *monzonite series*, which comprises rock types characterised by the presence of both orthoclase and a lime-bearing plagioclase, without either being greatly predominant.

The alkali series may be further sub-divided into :

- (1) A *potash series* with predominant potash-feldspar (orthoclase, microcline).

(2) A *soda series*, with a predominant soda-felspar (anorthoclase, soda-orthoclase, albite); or with a soda-lime felspar in combination with nepheline, and one or more soda-pyroxenes and soda-amphiboles.

(3) A *felspathoid series*, in which nepheline, sodalite, or allied felspathoid is largely developed, and felspar subordinate.

Although in nature there is a perfect gradation in each of these series from a most acid to a most basic type, yet for classificatory purposes it is convenient to break them up into three divisions corresponding to the Acid, Intermediate, and Basic groups as above defined. By doing this the following scheme of classification is obtained :

SCHEME OF CLASSIFICATION OF THE PLUTONIC ROCKS

	Alkali Series.			Monzonite Series.	Calc-alkali Series.
	Felspathoid Series.	Soda Series.	Potash Series.		
Acid group (With more than 66% Silica).		Soda granite Sub-family.	Potash-granite Sub-family.	Adamellite Sub-family.	Granodiorite Sub-family.
	GRANITE Family.				
Intermediate group (52-66% Silica).	Nepheline- ¹ syenite Sub-family.	Soda-syenite Sub-family.	Potash-syenite (Plauenite) Sub-family.	MONZONITE Family.	DIORITE Family.
	SYENITE Family.				
Basic group (with less than 52% Silica).	Nepheline- ¹ gabbro Sub-family.	Essexite Sub-family.	Shonkinite Sub-family.	Kentallenite Sub-family.	GABBRO Family.
	ALKALI-GABBRO Family.				

In the above scheme there are six main divisions or families, namely :

¹ The nepheline in these families may, of course, be partially or wholly represented by leucite, sodalite, or other felspathoid mineral.

The Granite Family.
 The Syenite Family.
 The Monzonite Family.
 The Diorite Family.
 The Alkali-gabbro Family.
 The Gabbro Family.

Of these, only the monzonite, diorite, and gabbro families are divisions of a single series. The remaining comprise several sub-families belonging to different series. Thus the granites are divisible into four sub-families, the syenites into three, and the alkali-gabbros into four.

A few points require explanation. The granodiorite sub-family includes granite-like rocks in which a lime-bearing plagioclase largely predominates over orthoclase. Such rocks are typically developed in the Andes of South America ("Andes-granite")¹ and in the corresponding Cordillera of the Western States of North America.² They occur also in the granite complexes of the southern uplands of Scotland, as for instance in the Criffel and Loch Dee masses of the Galloway area.³ The name "granodiorite" was first proposed in 1892 by G. F. Becker in conjunction with H. W. Turner and W. Lindgren. The work of these authors has defined the type as a light grey granitic rock in which soda-lime feldspars (basic oligoclase or andesine) largely predominate over orthoclase, the remaining essential constituents being quartz, biotite, and usually hornblende, together with sphene, apatite, and magnetite. It occurs in association with tonalite, diorite, and gabbro, and no doubt represents the extreme acid end of the gabbro-diorite series. According to Lindgren, the percentage of silica, which rises as high as 73 per cent., may fall as low as 59 per cent., and he makes the distinction from the more acid members of the diorite family depend on the orthoclase. But the fact is that most, if not all, diorites contain some small proportion of orthoclase; and, therefore, in order to avoid a

¹ O. Nordenskjöld, "Die krystallinische Gesteine der Magellansländer," *Svenska Exped. till Magellansländerna*, Stockholm, 1901, vol. i. p. 175.

² W. Lindgren, *Amer. Journ. of Sci.* Series 4, vol. iii. 1897, and Series 4, vol. ix. 1900, p. 269; H. W. Turner, *The Rocks of the Sierra Nevada*, 14th *Ann. Rep. U.S. Geol. Survey*, 1894, pp. 478 and 482.

³ The Silurian Rocks of Scotland, *Mem. Geol. Surv.*, p. 610. The need of a name for this type is shown by the fact that the dominant rock of these masses is sometimes described as a granite, sometimes as a tonalite.

multiplicity of series, it is better to make the percentage of silica the means of dividing the granodiorites from the diorites, and to call no rock with less than 66 per cent. of silica a granodiorite.

In using the name adamellite for the most acid division of the monzonite series corresponding in respect to acidity to the granodiorites of the calc-alkali series, and to the potash- and soda-granites of the alkali series, I am following Brögger's¹ suggestion. Used thus it includes many of the rocks known as biotite-granite, and hornblende-biotite-granite, which grade on the one side into the granodiorites and on the other into the alkali-granites, according as lime-bearing plagioclase or alkali-felspar predominates. For practical purposes Lindgren's² useful suggestion may, with a small modification, be adopted: namely, that a rock in which less than one-third of the felspar is orthoclase should be classed with the granodiorites (or with the diorites when the silica is less than 66 per cent.) and one with more than two-thirds of orthoclase with the granites (or syenites, when the silica is less than 66 per cent.). Only rocks in which between one-third and two-thirds of the total felspar is orthoclase would be referable to the adamellite (or monzonite) family.

For the basic division of the monzonite series, I have adopted Messrs. Hill and Kynaston's³ term kentallenite, instead of the more cumbersome synonym olivine-monzonite.

The comprehensive name "alkali-gabbro," to include the most basic divisions of the alkali series, was suggested to me by Mr. R. H. Rastall,⁴ of Christ's College, Cambridge. I have extended it to cover kentallenite, on account of the relation of that type to the shonkinites and essexites. In the shonkinites, orthoclase is the dominant felspar; in the essexites, a lime-bearing plagioclase (oligoclase or andesine) predominates, and

¹ Die Triadische Eruptionsfolge bei Predazzo, *loc. cit.* Christiania, 1895, p. 61. The name *adamellite* was used many years ago by Cathrein for the rocks now known as tonalite; but the name never came into general use, and there is, therefore, no valid objection to its being used in Brögger's sense.

² W. Lindgren, *Amer. Journ. of Sci.* 1900, p. 269.

³ J. B. Hill and H. Kynaston, "On Kentallenite and its relation to other Igneous Rocks in Argyleshire," *Q.J.G.S.*, vol. lvi. 1900, p. 531.

⁴ I am also much indebted to Mr. Rastall for the kind and ready response he has always made to my many calls on his intimate knowledge of petrological literature.

orthoclase (together with nepheline) is subordinate. In kentallinite, orthoclase and a lime-bearing felspar are equally developed. The position of the essexites in the alkali-series is justified by the part played by soda minerals (such as nepheline and the soda-pyroxenes) in their mineral composition and the consequent influence on their chemical composition. These characteristics distinguish the type from gabbro in which the feldspathic element is limited to a lime-bearing felspar (labradorite). The nepheline-gabbro sub-family embraces the types theralite, covite, and borolanite, representing the various combinations of orthoclase and plagioclase with nepheline (or leucite). Of these types, the theralite is intimately related to the essexite type, the covite to the shonkinite, the proportion of nepheline being the determining factor in each case.

It is to be noted that there is continuity of types both vertically (in series) and horizontally (from series to series). For example, not only do the granodiorites pass by decreasing silica-contents into diorites and thence into gabbros, but they pass also by decreasing lime and increasing alkalis into the adamellites and thence into the alkali-granites or granites proper. And similarly for each series and for each group. The divisions are adopted for convenience, but do not represent actual hard-and-fast lines existing in Nature.

Besides the types whose names have been used to designate the families in the above scheme, other points in the series have been fixed by specific names. Thus the acid division of the diorite family is generally known as *tonalite*, a name originally given by vom Rath to the quartz-mica-diorite of the Adamello Alps; while Brögger¹ proposes to use the name *banatite* for the corresponding acid division (62-66 per cent. SiO₂) of the monzonites. Again, *quartz-syenite* has been used for the more acid members of the potash-syenite family and *nordmarkite* (Brögger) for the corresponding acid members of the soda-syenite family.

Furthermore, varieties of the main types are distinguished in each family (1) by variations in the nature of the ferro-magnesian constituent, e.g. hypersthene-granite (*charnockite*), augite-syenite (*âkerite*), mica-, hornblende-, augite-, and hypersthene-diorite; (2) by the presence of some characteristic constituent (as, for example, zircon-syenite, eudialite-syenite, or arfvedsonite-

¹ *Loc. cit.* p. 61.

and ægirine-syenite); or (3) by the characteristic mode of crystallisation of a constituent (as in the *laurvikite* and *laurdalite* of Brögger, peculiar types of syenite and nepheline-syenite, in which the crystals of the dominant feldspar are rhomb-shaped). Although names, founded on mineralogical variation in constituents other than the feldspars, are of little service in classifying rock-types, they are often useful for descriptive purposes. They form no part, however, of the system of classification now undergoing discussion.

I propose now to show by a selection of analyses the chemical correlation of the main types in each series. This can be done graphically by Iddings' ¹ method, in which the molecular proportions of the silica are represented by abscissæ and those of the bases by ordinates, or by Brögger's ² modification of Michel-Levy's ³ method, in which the molecular ratios are plotted so as to give a characteristic picture of the rock-type. In Brögger's modification the molecular ratios are plotted on axes radiating from a central point O: four of these are formed by a vertical and a horizontal line intersecting at O, and four by lines forming an angle of 120° with the vertical and horizontal lines and also intersecting at O (see fig. 1). On the horizontal line the molecular proportion of the silica is laid off, ⁴ half to the right and half to the left. On the vertical line the alumina is laid off downward, the lime upward. The ratios for the iron (FeO) and magnesia are laid off on the oblique axes above, and the soda and potash below the horizontal line (see fig. 1). By joining the points thus obtained, a picture of the rock is produced, the shape of which at once shows the chemical character of the rock.

An example will make the matter perfectly clear. Suppose a diorite to have the chemical composition represented by the figures in column 1 of the following table, then the molecular proportions of the silica and the bases will be as shown in column 2, and the lengths to be laid off in millimetres will be as shown in column 3.

¹ J. P. Iddings, "The Mineral Composition and Geological Occurrence of certain Igneous Rocks in the Yellowstone National Park," *Bull. Phil. Soc.* Washington, vol. xi. 1890, pp. 191-220.

² Brögger, *Das Gangfolge des Laurdalits*, *loc. cit.*, Christiania, 1898, p. 257.

³ Michel-Levy, *Bull. d. serv. de la carte géol. de la France*, No. 57, vol. ix. (1897).

⁴ A convenient scale is 100 mm. to the unit.

CLASSIFICATION OF THE PLUTONIC ROCKS 251

	1	2	3
SiO ₂ .	56.52	94.2	47
Al ₂ O ₃	16.31	16	16
Fe ₂ O ₃ *	4.28	—	—
FeO	5.92	0.82	8.2
MgO	4.32	10.8	10.8
CaO	6.94	12.4	12.4
Na ₂ O	3.43	0.55	5.5
K ₂ O .	1.44	0.15	1.5
H ₂ O .	1.03		

Plotting these results in the manner described, the following figure is obtained :

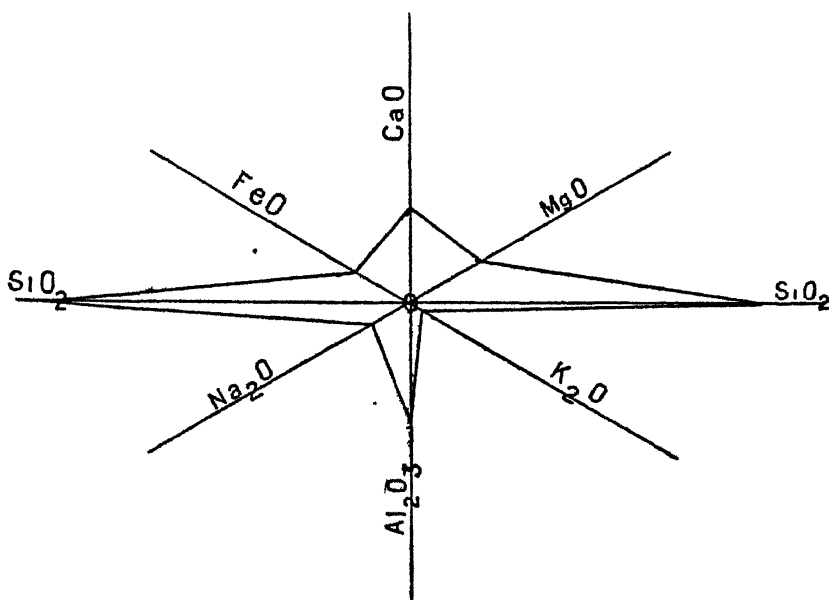


FIG. 1.

In the following pages the analyses of the best-known types in each series are given, each table of analyses being illustrated by a diagram on which the molecular proportions calculated from the same analyses have been plotted according to the method devised by Iddings. In selecting the analyses I have made use of Brögger's useful averages, calculated from well-known types of diorite, monzonite, potash-syenite (plauenite), and soda-syenite. In each other case as typical an example has

* The ferric oxide (Fe₂O₃) can be plotted on the FeO line and beyond the FeO ; but this is not essential to the present purpose.

been chosen as possible. In making the selection much assistance has been afforded by H. S. Washington's valuable collection of analyses.¹

THE CALC-ALKALI SERIES

Table of Analyses of Rock-types

	I.	II.	III.	IV.	V.
	Granodiorite.	Tonalite.	Quartz-diorite.	Diorite.	Gabbro.
SiO ₂ .	68.65	63.09	57.41	56.52	50.78
Al ₂ O ₃ .	16.34	18.89	17.71	16.31	17.16
Fe ₂ O ₃ .	.93	3.48	2.16	4.28	3.15
FeO.	1.48	2.02	5.01	5.92	7.61
MgO.	1.29	1.97	3.38	4.32	7.16
CaO.	3.07	6.18	6.73	6.94	10.28
Na ₂ O.	4.85	3.14	3.12	3.43	2.61
K ₂ O.	1.85	1.30	1.82	1.44	
H ₂ O.	.86	0.63	1.34	1.03	1.20
TiO ₂ .	.28	—	1.04	.25	
Other constituents { MnO, etc.)	.39	—	.54	.54	
	<u>99.99</u>	<u>100.70</u>	<u>100.26</u>	<u>100.98</u>	<u>99.95</u>

- I. Granodiorite, Indian Valley, Sierra County, California. W. F. Hillebrande. Quoted in H. S. Washington's *Chemical Analyses*, p. 179.
- II. Tonalite, Wistra, Carinthia, Austria. H. Krezmar. Quoted in H. S. Washington's *Chemical Analyses*, p. 193.
- III. Quartz-diorite, Tuolumne River, Amador County, California. W. F. Hillebrande. Quoted in H. S. Washington's *Chemical Analyses*, p. 237.
- IV. Diorite. Brögger's average of sixteen analyses of typical diorites. Die Triadische Eruptionfolge bei Predazzo, *loc. cit.* Christiania, 1895, p. 36.
- V. Gabbro. Broadford Boss, Skye. T. Baker. Quoted by Harker, The Tertiary Igneous Rocks of Skye, *Mem. Geol. Surv.*, 1904, p. 103.

It will be seen that, of these five analyses, the silica percentages of the two extremes place the rocks outside the limits of the Intermediate Group—that is to say, granodiorite is a member of the Acid, and gabbro a member of the Basic Group; while tonalite, quartz-diorite, and diorite are Intermediate rocks. In the diagram (fig. 2) the molecular proportions of the silica and bases are shown by the Iddings method. In this diagram it will be recognised as a general characteristic of the series that while the molecular proportion of lime is high, that of the alkalis is low, and especially is this true with regard to the potash, which is much lower than the soda. At the basic

¹ Chemical Analyses of Igneous Rocks, published from 1884 to 1900. *U.S. Geol. Survey Prof. Paper* No. 14, Series D and E, 1903.

end of the series the lime and magnesia ratios are nearly equal, but the magnesia falls more rapidly than the lime. The ferrous iron is parallel to and uniformly lower than the magnesia. The alumina is fairly constant throughout the series.

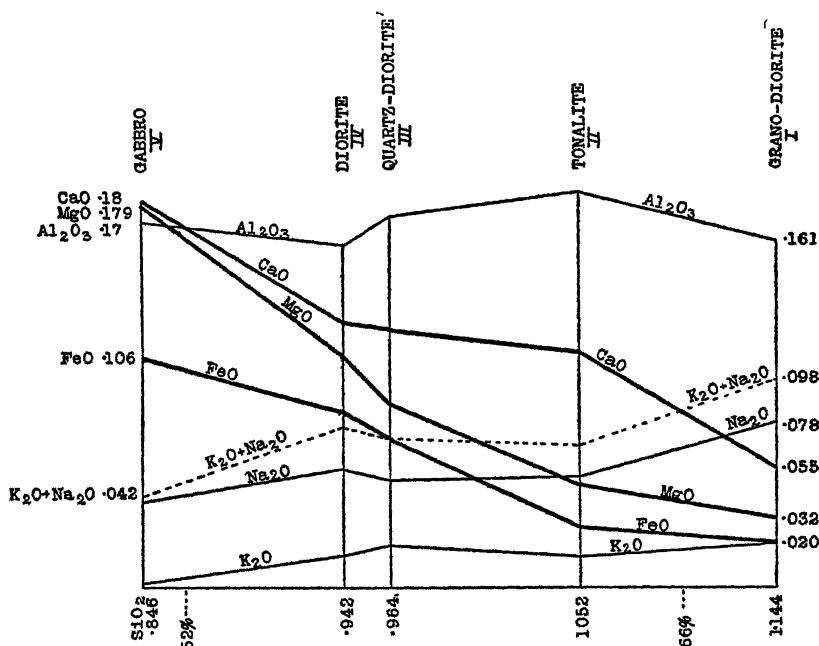


FIG. 2.—Diagram of the Molecular Proportions of the Calc-Alkali Series.

THE MONZONITE SERIES

Table of Analyses of Rock-types

	I.	II.	III	IV.	V
	Biotite-granite (Adamellite)	Adamellite.	Banatite	Monzonite.	Kentallenite (Olivine-monzonite).
SiO ₂	71.53	68.97	64.30	54.90	50.35
Al ₂ O ₃	13.55	14.80	17.89	17.44	15.76
Fe ₂ O ₃	1.20	3.29	4.75	9.64	{ 2.32 7.30 }
FeO	0.88	—	not det ^d		
MgO	1.45	1.15	1.12	3.26	7.40
CaO	3.21	3.82	3.98	6.96	10.12
Na ₂ O	2.61	2.46	3.84	3.50	2.75
K ₂ O	3.95	4.53	3.37	3.51	3.89
H ₂ O	1.75	0.70	1.60	1.23	.45
TiO ₂	—	—	—	—	.30
Other constituents (P ₂ O ₅ , MnO, etc.)	—	—	—	—	.74
	<u>100.13</u>	<u>99.72</u>	<u>100.84</u>	<u>100.44</u>	<u>101.38</u>

- I. Biotite-granite, Pfaffenberg, Riesengebirge. W. Herz, *vide* L. Milch, *Neues Jahrb.* B.B. xii. 1899, p. 162.
- II. Adamellite (Granite of Rosenbusch), Landsberg, Vosges Mountains. Unger. Quoted by Brögger, *Die Triadische Eruptionsfolge bei Predazzao*, *loc. cit.* p. 63.
- III. Banatite (Syenite of Bonney), Croft Hill, Charnwood Forest. E. E. Berry. *Q.J.G.S.* vol. xxxviii. 1882, p. 198.
- IV. Monzonite. Brögger's average of fourteen typical Monzonites, *loc. cit.* p. 51.
- V. Kentallenite (Olivine-monzonite of Brögger), Smålingen, Fahlen, Sweden. L. Schmelck, *vide* Brögger, *loc. cit.* p. 46.

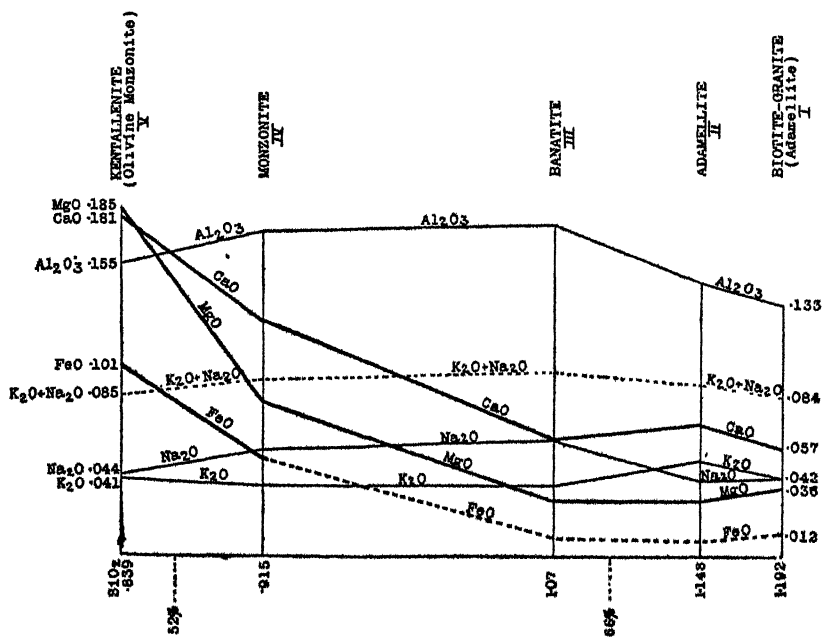


FIG. 3.—Diagram of the Molecular Proportions of the Monzonite Series.

Of these five analyses, the first two have silica percentages above 66, and the last a silica percentage below 52. In other words, Nos. I. and II. are Acid rocks, No. V. belongs to the Basic division, while Nos. III. and IV. are members of the Intermediate Group.

The Iddings diagram (fig. 3) shows that the character of the Monzonite Series is not unlike that of the Calc-alkali Series, the chief difference being determined by the higher proportion of alkalis, due to the increase in potash, which in this series nearly equals the soda. This is the result of the greater pro-

CLASSIFICATION OF THE PLUTONIC ROCKS 255

portion of orthoclase as compared to plagioclase in the members of the Monzonite Series. It will be observed that the alumina curve falls slightly at the acid end of the series.

THE POTASH DIVISION OF THE ALKALI SERIES

Table of Analyses of Rock-types

	I.	II.	III.
	Potash-granite.	Potash-syenite (Plauenite).	Shonkinite.
SiO ₂	70.17	60.57	47.85
Al ₂ O ₃	15.07	15.85	13.24
Fe ₂ O ₃	.88	8.23	2.74
FeO	1.79		
MgO	1.11	2.59	5.68
CaO	1.13	4.44	14.36
Na ₂ O	2.69	2.13	3.72
K ₂ O	5.73	6.02	5.25
H ₂ O	.88	1.06	2.74
TiO ₂	.41	0.53	—
Other constituents (P ₂ O ₅ , MnO, etc.)	.75	—	2.42
	<u>100.61</u>	<u>101.42</u>	<u>100.65</u>

- I. Potash-granite, Lamorna, Cornwall. W. Pollard, *Geology of Land's End*, *Mem. Geol. Surv.*, 1907, p. 59.
- II. Potash-syenite (Plauenite of Brögger). Brögger's average of three analyses (namely, Plauen near Dresden, Biella in Piedmont, and Reichenstein in Lower Silesia), *loc. cit.* p. 31.
- III. Shonkinite, Poohbah Lake, Rainy River District, Ontario. F. L. Ransome. Quoted by Rosenbusch, *Elemente der Gesteinslehre*, 1898, p. 176.

Of the three rocks represented by these analyses, the shonkinite belong to the Basic and the potash-granite to the Acid Group, while the syenite falls within the limits of the Intermediate Group. Referring to the diagram (fig. 4), the Potash Series is in general characterised by the fact that the potash is present in nearly equal molecular proportion to the soda. The sum of the two bases remains practically constant throughout the series. Lime is very high in the shonkinites (in the example chosen 14.4 per cent.), but falls rapidly as the syenites are approached, and this base is scarcely represented in the muscovite-granites which form the extreme acid end of the series. There is a similar rapid diminution in the proportions of magnesia and iron, although these bases are not present in the shonkinites in such

great proportions as the lime. The alumina proportion culminates in the syenites.

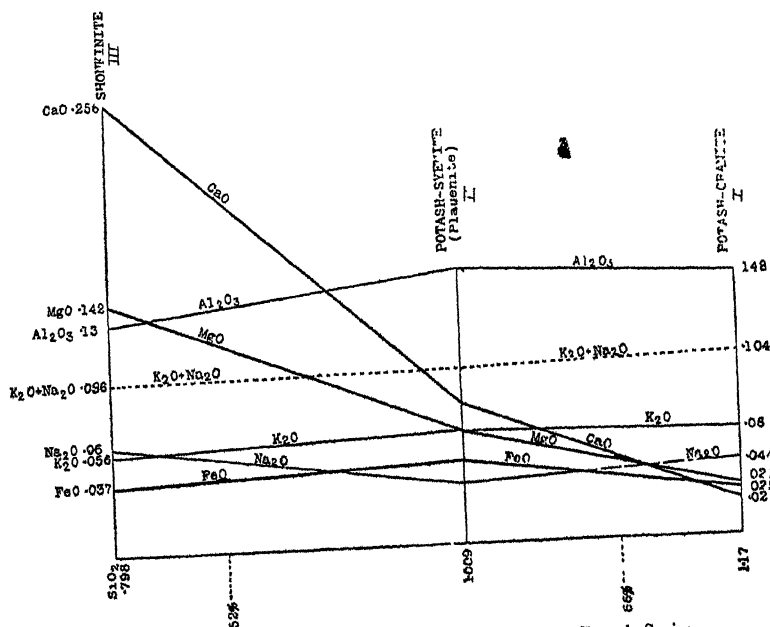


FIG. 4.—Diagram of the Molecular Proportions of the Potash Series.

THE SODA DIVISION OF THE ALKALI SERIES

Table of Analyses of Rock-types

	I.	II.	III.	IV.
	Soda-granite.	Nordmarkite.	Soda-syenite.	Essexite.
SiO ₂ . . .	71'65	64'04	58'32	47'94
Al ₂ O ₃ . . .	13'04	17'92	18'23	17'44
Fe ₂ O ₃ . . .	2'79	0'96	6'83	6'84
FeO . . .	1'80	2'08		6'51
MgO . . .	tr.	0'59	1'31	2'02
CaO . . .	tr.	1'00	4'12	7'47
Na ₂ O . . .	6'30	6'67	5'70	5'63
K ₂ O . . .	3'98	6'08	3'84	2'79
H ₂ O . . .	1'10	1'18	1'02	2'04
TiO ₂ . . .	—	0'62	'54	0'20
Other constituents { (P ₂ O ₅ , MnO, etc.)	—	0'23	'85	1'04
	<u>100'66</u>	<u>101'37</u>	<u>100'76</u>	<u>99'92</u>

I. Soda-granite ("Natron-granit" of Brögger), Hougnavatten, Loughenthal Norway. L. Schmelck. Quoted in Washington's *Chemical Analyses*, p. 155.

- II. Nordmarkite, Tonsenäs, Christiania. Quoted by Brögger in Das Gangfolge des Laurdalits, *loc. cit.* Christiania, 1898, p. 375.
- III. Soda-syenite ("Natron-syenit" of Brögger). Brögger's average of ten analyses of typical soda-syenites (akérite, laurvikite, augite-syenite, etc.). Die Triadische Eruptionsfolge bei Predazzo, *loc. cit.* Christiania, 1895, p. 33.
- IV. Essexite, Salem Neck, Essex County, Massachusetts, U.S. M. Dittmar, *vide* Rosenbusch's *Elemente der Gesteinslehre*, 1898, p. 172.

In the Soda Series, the Basic Group is represented by the essexites, the Acid Group by the soda-granites (Brögger), and the Intermediate Group by the soda-syenites and nordmarkites (Brögger). Nordmarkite is thus a quartz-soda-syenite.

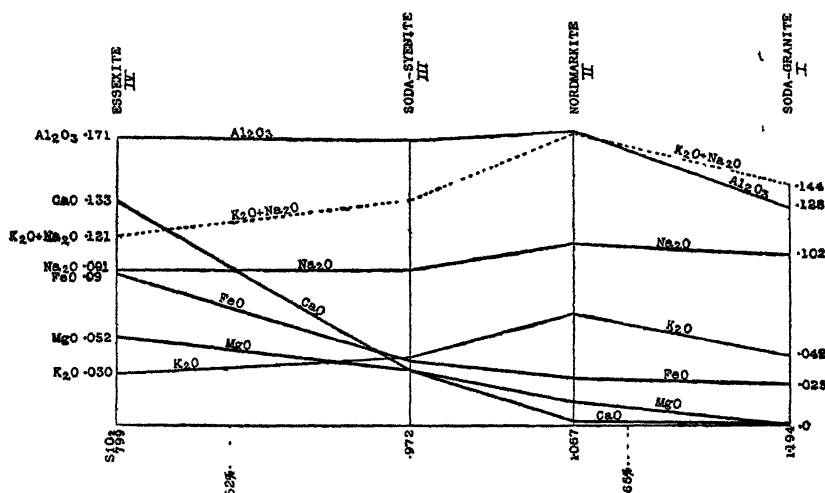


FIG. 5.—Diagram of the Molecular Proportions of the Soda Series.

The general characteristic of the series, as shown by the Iddings diagram (fig. 5), is the high alkali-content, especially in regard to the soda, which is present in all its members in about double the molecular proportion in which potash is present. Lime is fairly high in the essexites, but rapidly diminishes with increasing acidity, while magnesia is remarkably low even at the basic end.

The alkalis reach their maximum in the nordmarkites, a total of 12.7 per cent. being contained in the example chosen. The proportion of alumina is constant at a fairly high level in the essexite, soda-syenite, and nordmarkite types, but falls rather sharply towards the acid end of the series.

THE FELSPATHOID DIVISION OF THE ALKALI SERIES

Table of Analyses of Rock-types

	I. Nepheline- syenite.	II. Covite.	III. Theralite (Covite)
SiO ₂ . . .	55.78	49.70	44.65
Al ₂ O ₃ . . .	21.34	18.45	13.87
Fe ₂ O ₃ . . .	2.13	3.39	6.06
FeO	2.86	4.32	2.94
MgO	0.78	2.32	5.15
CaO	1.93	7.91	9.57
Na ₂ O	7.92	5.33	5.67
K ₂ O	5.83	4.95	4.49
H ₂ O	1.29	1.34	3.06
TiO ₂	0.55	1.33	0.95
Other constituents (P ₂ O ₅ , MnO, etc.)	.34	.40	3.52
	<hr/> 100.75	<hr/> 99.44	<hr/> 99.93

I. Nepheline-syenite. Brögger's average of thirteen analyses of typical nepheline-syenites. *Die Triadische Eruptionsfolge bei Predazzo, loc. cit* Christiania, 1895, p. 27.

II. Covite, Magnet Cove, Arkansas, Washington. *Journ. of Geology*, vol. ix. 1901, p. 612.

III. Theralite (Covite), Gordon's Butte, Crazy Mts., Montana. W. F. Hillebrande. Quoted in Washington's *Chemical Analyses*, p. 347.

Since quartz and the feldspathoids are antithetical, the Nepheline Series has representatives only in the Basic and Intermediate groups, namely the theralite, the covite, and the nepheline-syenite types. The theralite of Gordon's Butte in the Crazy Mountains of Montana, the analysis of which is quoted in the above table, has generally been considered as the type rock; but this rock contains too much orthoclase to tally with Rosenbusch's definition of a theralite, namely, as a plagioclase-nepheline rock. It appears rather to approximate, as regards mineral composition, to Washington's Magnet Cove type (covite); but is somewhat more basic than this.

It will be noticed in the diagram (fig. 6), that the alumina, which is rather low in the most basic representative, reaches a very high level in the nepheline-syenite (21.3 per cent.); and the same is true of the alkalis (total=13.75 per cent.). The molecular proportion of the soda is about double that of the potash at both ends of the series. Lime and magnesia are high at the basic end, but very low in the nepheline-syenites.

CLASSIFICATION OF THE PLUTONIC ROCKS 259

The affinities of the various members of a continuous series or of members of different series in the same group, are also

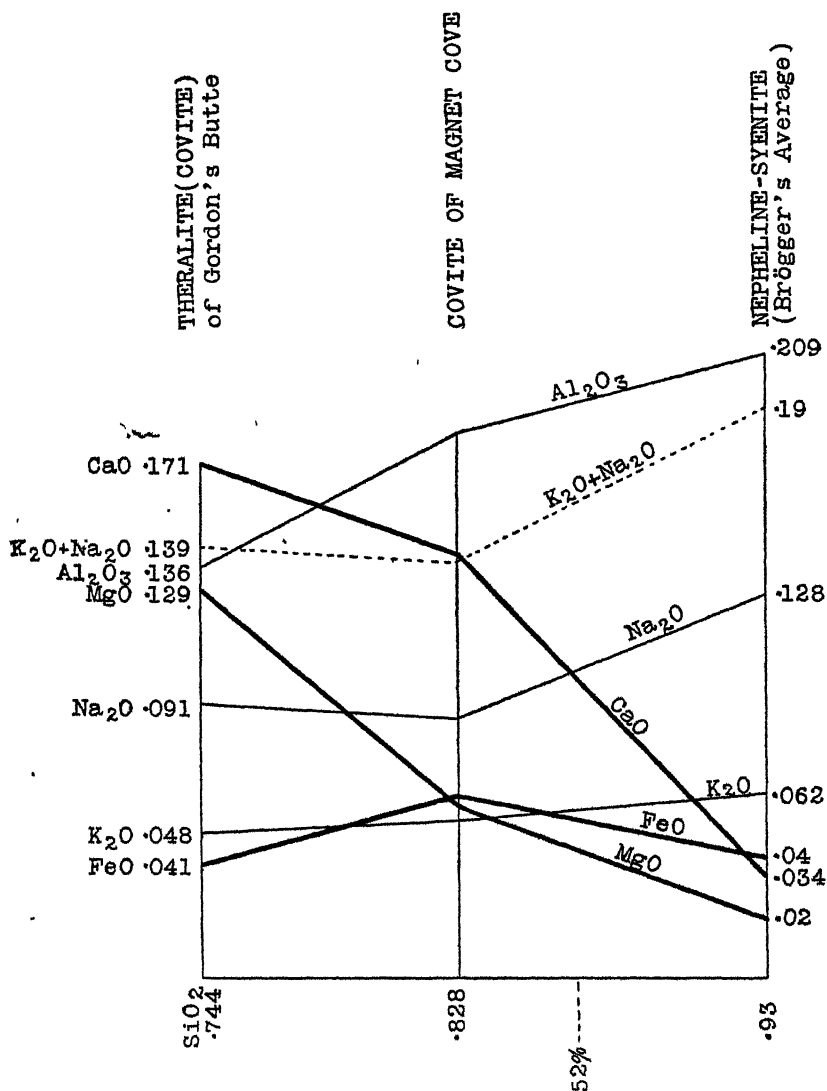


FIG. 6.—Diagram of the Molecular Proportions of the Nepheline Series.

well brought out by Brögger's figures. Exigencies of space prohibit the comparison of a large number of these figures; but it will suffice to trace their variation through the chief members

of a series, and from one series to another. Below are the figures obtained by plotting the molecular proportions (in the manner already explained) of a granodiorite and of a gabbro, the analyses of which are given on page 252. The increase in

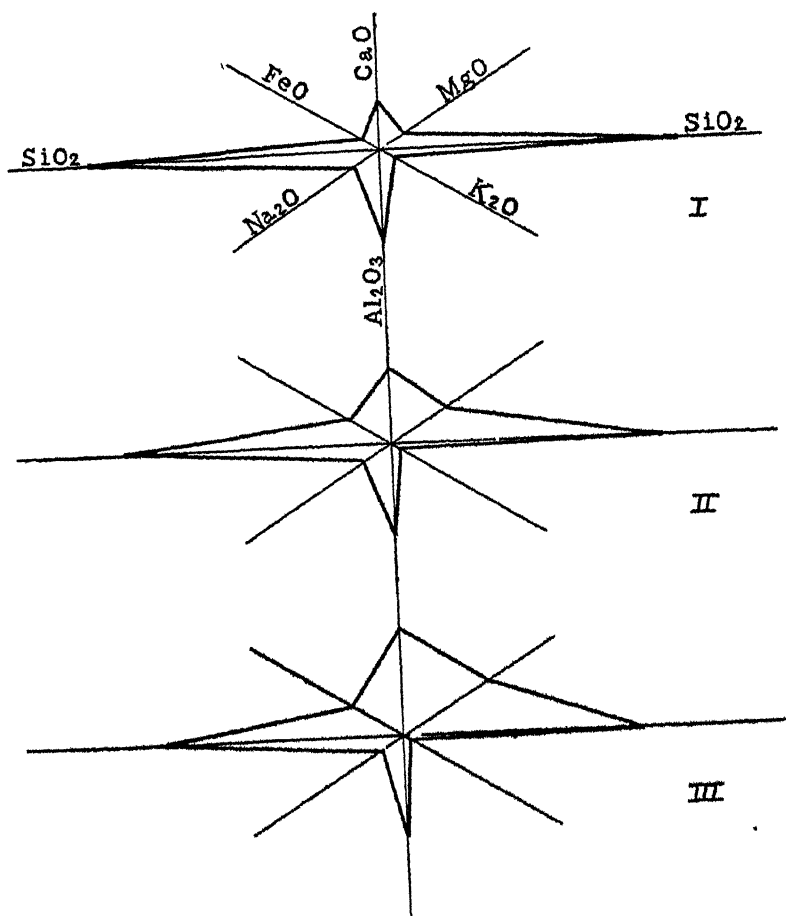


FIG. 7.

I. Granodiorite. II. Diorite. III. Gabbro.

lime and magnesia, and the decrease in alkalis, with increasing basicity, are well shown. The three figures on the opposite page are plots of the molecular proportions of a diorite, a monzonite, and a soda-syenite, the analyses of which are given on pages 252, 253, and 256. These illustrate the variation from series to series

in the same (in this case, the Intermediate) group. The figures show the comparatively high lime, magnesia, and iron in the representative of the Calc-alkali Series, the greater proportion of

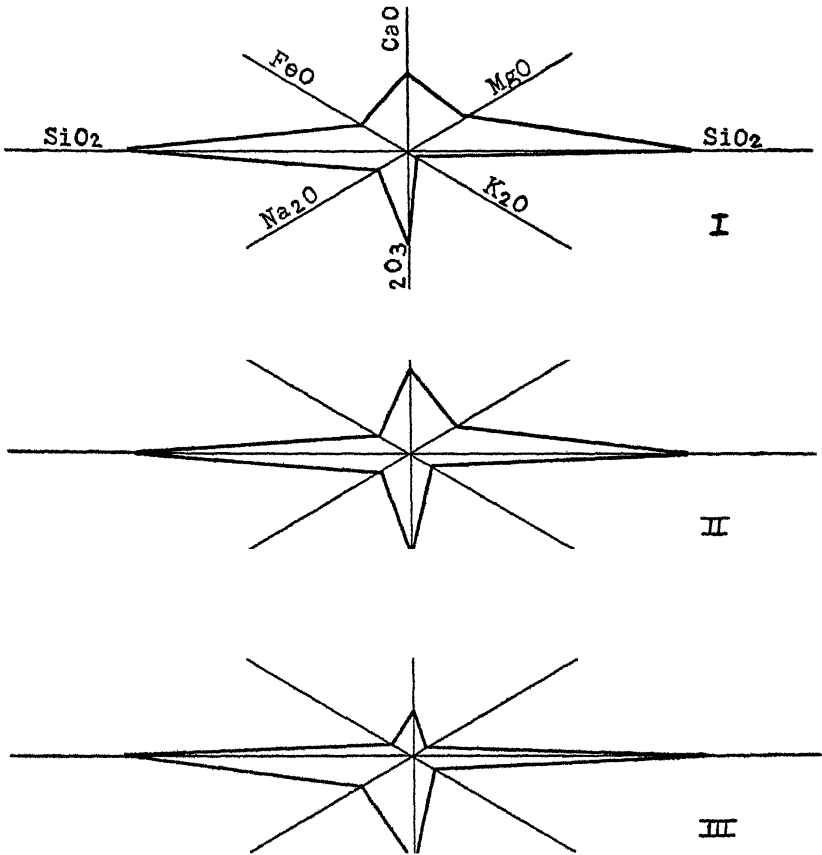


FIG. 8.

I. Diorite. II. Monzonite. III. Soda-syenite.

alkalies without diminution in lime in the Monzonite, and the greatly increased alkalies with diminished lime, magnesia, and iron in the representative of the Alkali Series.

If a further justification for the division of the igneous rocks into series be required, there is the fact that although the

assemblage of rocks in any given area of the earth's surface may range in composition from quite basic to very acid types, they usually have chemical characters in common which place them in a definite series. Thus, for example, the whole chain of the Andes in South America and the corresponding coast-ranges (the Sierra Nevada, etc.) and the main chain of the Rocky Mountains in the Western States of North America are characterised by the occurrence of rocks belonging to the calc-alkali series, namely, granodiorites and diorites among the plutonic rocks, and their corresponding volcanic equivalents, the andesites; while the hill-ranges east of the Rockies (in Montana and other Central States¹), and of the Andes (e.g. in the Argentine and Paraguay) present types progressively rich in alkalis. Again, over large areas of the eastern portion of the African Continent and in the adjacent islands, the prevailing types belong to the alkali series.² In smaller areas, well named by Prof. Judd³ *petrographical provinces*, the prevalence of types belonging to a definite series is also shown. Thus the Kola Peninsula⁴ and New England⁵ may be quoted as examples of an alkali-province. On the other hand, no better example of the occurrence in a limited area of members of the calc-alkali series can be given than the plutonic complex of Garabal Hill, near Loch Lomond,⁶ which comprises a rock series ranging from hornblende-biotite-granite through tonalite and diorite to peridotites, or that of the Loch Dee mass in the Galloway area (granodiorite, tonalite, and diorite),⁷ or again the rocks composing the Brocken granite *massif* in

¹ Pirsson, "Petrographical Province of Central Montana," *Amer. Journ. of Sci.* vol. xx. 1905, p. 36.

² Lacroix, "Les roches alcalines caractérisant la Province pétrographique d'Ampasindeva," *Nouv. Arch. d. Muséum*, 4^{me} sér. vols. iv. and v., Paris, 1902, 1903. See also an interesting paper on this subject by G. T. Prior, "Comparison of Volcanic Rocks from the Great Rift Valley with Rocks from Pantellaria, the Canary Islands, Ascension, St. Helena, Aden, and Abyssinia," *Journ. of the Min. Soc.* vol. xiii. 1903, p. 228.

³ J. W. Judd, "On the Ancient Volcano of the District of Schemnitz," *Q.J.G.S.* vol. xxxii. 1876, p. 292.

⁴ W. Ramsay and V. Hackman, "Das Nephelinsyenitgebiet auf der Halbinsel Kola," *Fennia*, vol. xi. No. 2, Helsingfors, 1894.

⁵ H. S. Washington, "The Petrographical Province of Essex County, Mass.," *Journ. of Geol.*, vol. vi. 1898, p. 787.

⁶ Dakyns and Teall, *Q.J.G.S.* vol. xlviii. 1892, p. 104.

⁷ *Loc. cit.*

the Harz¹ (hornblende-biotite-granite, tonalite, diorite, and gabbro).

It was Rosenbusch² who, by extending Durocher's³ hypothesis of the liquation of the original earth-magma into two zones, consisting of an upper acid, and a lower basic magma, first formulated the theory that the plutonic rocks have been produced from a number of partial magmas which originated by differentiation from a homogeneous earth-magma (*Urmagma*). From the magma-basins in which these earliest separated products had accumulated, subsequent differentiations took place, perhaps under the influence of the pressure due to earth movements in a sinking area, as postulated by Brögger for the Christiania district.⁴

According to Rosenbusch's theory, the first step in rock evolution consisted in the differentiation of the original earth-magma into two classes of secondary magmas: (1) those rich in alkalis, which he designates the nepheline-syenite (*foyaïtische*) and theralite magmas, and (2) those poorer in alkalis, but richer in lime and magnesia, which he called the granitodiorite and gabbro-peridotite magmas. This division corresponds almost exactly to the alkali and calc-alkali rock-series, and Rosenbusch applies these names to his magmas in the last edition of the *Mikroskopische Physiographie* (vol. ii. 1907).

The recognition of the existence of an intermediary or connecting magma, giving rise to the monzonite rock-series, the members of which present affinities sometimes to the alkali and sometimes to the calc-alkali series, we owe to Brögger's classic work in Southern Tyrol and in the Christiania district.

In conclusion, it may be claimed that there is in the classification discussed in these pages a freedom from the artificiality of the "quantitative system" proposed by the distinguished American petrographers named in the opening sentence of this

¹ Lossen, "Ueber Augitführende Gesteine aus dem Brocken Massif im Harz," *Zeits. d. Geol. Gesell.* vol. xxxii. 1880, p. 206.

² H. Rosenbusch, "Ueber die chemischen Beziehungen der Eruptivgesteine," *Tschermak's Min. Pet. Mitth.* vol. xi. 1890, p. 144.

³ J. Durocher, "Essai de pétrologie comparée ou recherches sur la composition chimique et minéralogique des roches ignées sur les phénomènes de leur émission et sur leur classification," *Ann. d. Mines*, ser. 5, vol. xi. 1857, p. 217.

⁴ Die Triadische Eruptionsfolge bei Predazzo, *loc. cit.*, Christiania, 1895, p. 177, and "The Basic Eruptive Rocks of Gran," *Q.J.G.S.* vol. I. 1894, p. 15.

paper; moreover, that it is on the lines along which petrology has been advancing during the past two decades, and possesses a very great advantage in retaining, for its chief families, names that are in general use in the languages of all countries where petrology is studied, without divorcing them from the significance which they have acquired by custom or consent.

ARTIFICIAL MODIFICATIONS IN THE COLOURING OF BIRDS

By R. LYDEKKER

DURING the last few years a number of experiments have been undertaken in America by Mr. C. W. Beebe, of the New York Zoological Society's Gardens, with a view of ascertaining the amount of alteration which can be effected in the colour or colour-pattern of the plumage of birds by means of conditions of temperature and humidity differing more or less markedly from the normal. The results are not only remarkable, but of great interest and importance with regard to the origin and stability of local forms and species, and consequently on what amount or kind of variation ought to be recognised as of racial or specific value. A further series of experiments has been instituted with a view of ascertaining the effect on the nuptial plumage by keeping birds under somewhat abnormal conditions, and especially by preventing their breeding.

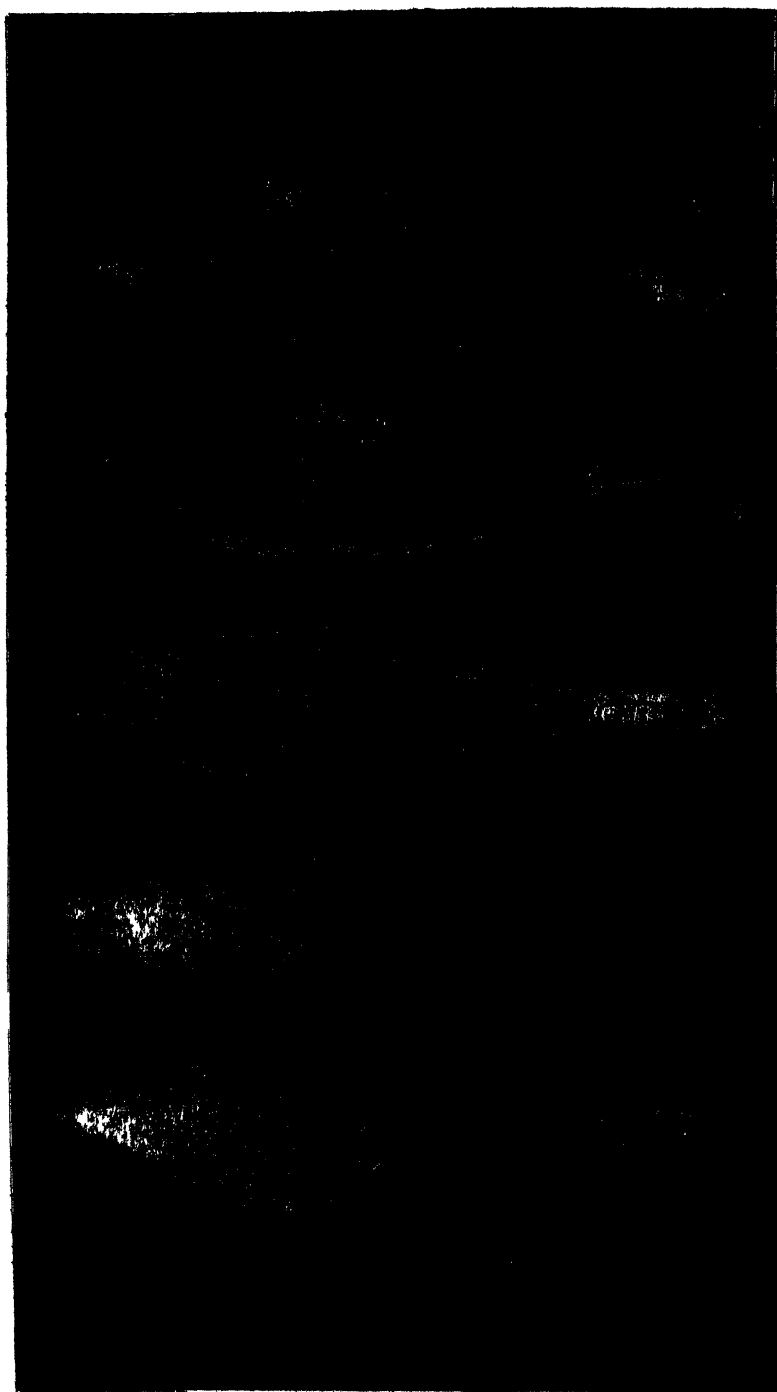
In regard to the experiments dealing with the effects of humidity, it is a well-known fact that many kinds of terrestrial vertebrates inhabiting warm, moist districts display a much darker type of pigmentation in their hair, feathers, or scales than is the case with individuals of the same or kindred species dwelling in drier localities. Correlated with this darkening of the epidermal covering is frequently a difference in the matter of size, extending in some instances to the entire body, but in other cases restricted to some particular region, as exemplified by the lengthening or shortening of the tail and wing feathers in birds.

When the dark humid forms are isolated by barriers of some description from their normal representatives, they are invariably recognised by naturalists as distinct species or races, according to the degree of differentiation shown; while even in cases where a gradation can be traced from the normal to the melanistic phase, each extreme type of the series (as well as sometimes a certain proportion of the intermediate stages) is admitted to be worthy of recognition by the systematist.

In addition to this humid melanism there is known to exist a tendency towards blackness among certain species in particular regions; the blackness in this instance manifesting itself only in a certain percentage of individuals. This "dichroism" is particularly well exemplified in the case of the snipe. Although this species ranges over a very large portion of the Old World, out of fifty-five melanistic examples known in collections (the so-called *Gallinago sabinei*), thirty-one were killed in Ireland, twenty-two in England, one in Scotland, and one on the Continent. Here, then, we have an interesting case of the localisation and restriction to a humid region of an occasional melanistic phase. An analogous instance is presented by the relative abundance of black leopards in the warm damp forests of the Malay countries, from which probably come nine out of every ten examples of the black phase.

Next comes what may be termed casual or sporadic melanism, in which individual members of a species or race turn black altogether apart from locality or station. This type of melanism is extremely common among cage-birds, and has been attributed to special kinds of food, hemp-seed for example. According, however, to Mr. A. G. Butler, food has nothing to do with the matter; and the phenomenon, which is most developed in old birds, is more probably due to unusual constitutional vigour on the part of such individuals. In this latter opinion Mr. Butler expresses a view identical with one suggested by Prof. Einar Lönnberg, of Upsala, to explain the development of blackness in the adult males of many species of ruminant mammals; a development shared in certain cases by the females.

Although there is no reference to it in Mr. Beebe's paper, mention may also be made of what seems to be yet another type of natural melanism. Some months ago I described—in the Zoological Society's *Proceedings*—a tiger-cat from the Congo forest as a distinct local race, characterised by its dusky colouring, of a well-known species whose normal hue is either rufous or grey. Shortly after the publication of the paper a specimen of the same species living in the Society's menagerie changed from red to dusky, thus demonstrating the slight value to be attached, from a taxonomic point of view, to these colour-phases. That the dusky phase is, however, connected with humidity cannot for a moment be doubted; but it has yet to be shown whether the change from red to dusky is a character common to the



species generally, or whether it occurs only in certain parts of the forest district, and if so whether it is restricted to certain individuals, and also whether it is permanently retained in these. As it certainly does not take place on the attainment of full maturity (as in the case of the above-mentioned ruminants), it may well be termed senile melanism, with or without the prefix local or individual.

The experiments relating to melanism carried out by Mr. Beebe on birds in the New York Zoological Park were conducted on three species, and are described and figured in the first number of *Zoologica*, a serial recently started by the New York Zoological Society. The mean annual humidity of New York is 73 per cent. ; but the average humidity of the chamber in which the experiments were conducted was no less than 84 per cent. During the warmer months the temperature averaged 68 degrees, or that of the city generally ; but from October to March it varied from 60 to 72 degrees.

The first experiment was conducted on a trio of young wood-thrushes (*Hylocichla mustelina*), taken from the nest, and known to be the offspring of normally coloured parents. One of the trio died in an early stage of the experiments ; the second was kept in a normal atmosphere, and differed in no respect from a wild-bred bird ; while in the third, after a two years' sojourn in the humid chamber, the black tips to the breast-feathers were found to have become more than double the usual size.

The second experiment was conducted on the white-throated sparrow (*Zonotrichia albicollis*), of which two examples were caught in the autumn of 1901, and one placed in a normal and the other in a super-humid atmosphere. As in the other experiments, the two birds had a precisely similar diet, from which hemp-seed was excluded. In May 1902, the two sparrows presented no noticeable differences ; but a year later a radical change had taken place in the one kept in the humid chamber ; the general effect being as though a dark veil had been drawn over the normal markings, the whole tone of the plumage being blackish, in place of the under-surface of the head and the abdomen being almost white.

The white-throated sparrow has a very extensive geographical range in eastern North America, but everywhere presents the same type of colouring ; this lack of local variation being probably due to its migratory habits, rendered necessary

by the fact that its breeding range is mainly restricted to the area extending from New York and Michigan northwards to Labrador and Hudson Bay.

The third, and perhaps most remarkable, example relates to the so-called Inca or scaly dove (*Scardafella inca*), one of two species whose united range extends from the south of Texas and Arizona in the north to Brazil in the opposite direction. The typical bird extends from Texas to Nicaragua, where, and in Honduras, it is replaced by the race known as *S. i. dileucus*. Farther south, on the island of Marguerita and the Venezuelan coast, we find a second species, *S. ridgwayi*, which, in its turn, is replaced in Brazil by the race *S. r. brasiliensis*. Within their respective localities these four forms appear to be perfectly constant in the matter of general colouring and markings; *S. inca*, to put it shortly, having almost uniformly light under-parts, while in *S. ridgwayi* the same region is marked by a number of transverse dark flecks, which also extend on to the wing-feathers. *S. i. dileucus* and *S. r. brasiliensis*, it should be added, are intermediate in these respects between the Texan and Venezuelan birds. As regards minor details of marking, such as the relative amounts of black and white on the wing-feathers, there is, however, great individual variation even among birds from identical localities.

When specimens of the typical Inca dove were confined in a super-humid atmosphere, a radical change in the pigmentation was observed to take place with each succeeding annual moult; while a change apparently similar in extent and direction resulted from an artificially induced monthly replacement of the plumage. At the first annual moult the bird was brought very close to the *Scardafella inca dileucus* type; at the second moult the colour-pattern approximated either to the Brazilian or the Venezuelan phase of *S. ridgwayi*; while after subsequent changes the plumage became more profusely and heavily marked than in any of the normal wild representatives of the genus. Intrinsically the change appears at first to be a segregation and intensification of the black pigment, resulting in an extension and clearing-up of the white or whitish areas. Next ensues a brief period of equilibrium, succeeded by such an increase of the black pigment that the latter begins to encroach upon the light areas on the quill and tail feathers, till eventually

all traces of the white vanish. Coincident with this intense blackening of the plumage there occurs an increased development of black pigment in the choroid coat of the eye. Finally, when the concentration of the dark pigment has attained a certain stage, a change occurs in its colour, which is transformed from dull dark brown or black to brilliant iridescent bronze or green, which reaches its maximum development on the wing-coverts and inner secondary quills, or just in those regions where iridescence of a similar type is met with in many genera of tropical and subtropical doves. All these remarkable changes, it is important to mention, take place by continuous variation, there being no colour-change unaccompanied by a moult.

Some similar changes have been observed to take place in the case of the yellow-rumped weaver-finch (*Munia flaviprymnus*) of Australia, when kept in confinement, the throat and upper part of the breast developing dark mottlings similar to those which occur in a more marked degree in the chestnut-breasted weaver-finch (*M. castaneithorax*), accompanied by a darkening of the sides of the head. In this instance it has been suggested that the yellow-rumped bird is merely the desert phase of the chestnut-breasted species; and that confinement in a relatively more humid atmosphere induces reversion towards the original type.

As regards the conclusions to be drawn from these experiments, Mr. Beebe points out in the first place that the distribution of the pigeons in America indicates the group to be essentially a tropical one in that continent. Hence, in the case of the Inca dove the alteration in colour induced by humidity is a reversion to the ancestral type. Further, in the case of the individual Inca doves, it is clear that the apparently atavistic characters are acquired. This leads to the question whether, if these melanistic doves were to breed, would the acquired characters be inherited?

Another point which may be raised is that the experiments, so far as they go, indicate that natural selection has nothing to do with such phenomena as protective coloration. This, however, the author thinks, is not borne out by the facts; for even among the four or five individuals upon which the experiments were made, there was a noticeable difference in the response to the abnormal conditions.

The next question is to what extent, if any, these experi-

ments should alter the current practice with regard to taxonomy. In other words, does the fact that *Scardafella inca* can be modified by humidity into a form closely resembling *S. ridgwayi*, or indeed into what may be called an *ultra-ridgwayi* form, render it necessary that we should regard these two "species" as identical? To this Mr. Beebe replies in the negative.

"The crucial point," he writes, "seems to be that, while we should use this evidence to the utmost in unravelling the intricate processes of evolution and in understanding the past history of the wild-living forms, or, as we call them, species and sub-species, as now defined, yet to alter our entire list of species, discarding all forms which are ontogenetically interchangeable under experiment or in a new environment, by analysis or synthesis as the case may be, is no more reasonable than to discard a genus of living creatures because palæontology reveals more delicate gradations between it and a second living group, isolated by the present conditions of life."

On the other hand, Dr. D. S. Jordan, in an article on "Geminate Species" published in the February number of the *American Naturalist*, remarks that "the ontogenetic species—groups in which many individuals are simultaneously modified in the same way by like conditions of food or climate—show no permanence in heredity. Such forms, however strongly marked, would therefore have no permanent place in taxonomy. The recent studies of Mr. Beebe on the effect of moist air in giving dusky colours to birds serve to illustrate the impermanence of the groups or sub-species characterised by dark shades of colour in regions of heavy rainfall."

Dr. Jordan, if I understand him rightly, thus advocates the uniting of *Scardafella ridgwayi* with *S. inca*, and he would also refuse to recognise the species or races which have been named on the evidence of pale or dusky forms adapted respectively to arid or humid climatic conditions. A revision of our species and sub-species carried out on such lines would, however, involve endless difficulties and confusion.

I pass on to a notice of Mr. Beebe's second set of experiments dealing with the seasonal colour-changes in birds, or, in other words, the assumption by the males of many species of a special nuptial plumage at the beginning of the breeding season. These experiments, of which an account will be found in the January number of the *American Naturalist*, are at

present only in their infancy, but seem to promise results of exceptional interest.

During a recent summer Mr. Beebe put under careful observation several male specimens of the scarlet tanager (*Piranga erythromelas*) and the bobolink (*Dolichonyx oryzivorus*), which had not been allowed to breed, and were therefore, despite the fact that it was somewhat late in the season, in full breeding-livery, as well as in the height of vocal and physical condition. In the course of a few days the supply of light was gradually cut off, while the amount of food given to each bird was slightly increased; and a month later, when under normal circumstances the breeding-dress should have been exchanged for the duller winter garb, no change of any kind took place in the plumage, not a feather being shed. In fact, the birds entirely skipped the autumnal moult, without any apparent effect on their well-being, so far as appearance went, all being in perfect health, although showing symptoms of inactivity owing to their abnormal fatness.

Occasionally one of the birds was brought into the light and a few mealworms added to its diet, when it invariably resumed its song, so that the spring-notes of the tanager were heard even in mid-winter. This portion of the experiment could not, however, be indefinitely repeated, for the period of song seemed, as under normal conditions, to be definitely limited.

Early in the following spring some of both tanagers and bobolinks were gradually brought under normal conditions and activities, with the result that the spring moult speedily took place. In the case of the tanagers, the old scarlet and black feathers were replaced by new ones of the same colour, while in like manner the bobolinks moulted into a livery of buff, cream, and black. In fact, the moult was without exception from the breeding to the breeding-livery, and not from the brilliant nuptial to the dull winter plumage.

"I think," concludes Mr. Beebe, "we thus have proof that the sequence of plumage in these birds is not in any way predestined through inheritance bringing about an unchangeable succession, in the case of the tanager, of green, scarlet, green, year after year, but that it may be interrupted by certain external factors in the environmental complex.

"The further significance of these results I leave to others, or until I have more complete data, checked by results derived

from control of the other factor of the environment. It would be worse than useless to formulate any theories at the present incomplete stage of the experiments."

Here, then, we must perforce leave the matter, not, however, without the expression of the hope that Mr. Beebe will be enabled to continue these experiments, from which important additions to our knowledge of the ætiology of colour-development in birds are almost sure to eventually accrue.

GASEOUS REFRACTIVE INDICES

By C. CUTHBERTSON

Fellow of London University, University College

LIGHT, which travels with a velocity of 186,300 miles a second through empty space, is retarded in passing through transparent material bodies. If the surface of the medium is not normal to the direction of the beam of light, this direction is changed, and the light is said to be refracted. The refractive index of the material is defined as the ratio of the sine of the angle of incidence to that of the angle of refraction; but it is simpler to think of it as the ratio of the velocity of light in vacuo to its velocity in the medium. This ratio is usually indicated by the letter μ ; and, since the velocity of light in vacuo is usually greater than in a material medium, μ is a number greater than 1. In water it is about 1.33, in crown glass about 1.5, and in the diamond 2.42. In passing from one medium to another the frequency of the periodic disturbance which constitutes light remains constant, and, consequently, the change of velocity must be due to a change in the distance between two similar states of the disturbance, which is called the wave length. Hence, the refractive index, in its simplest terms, is the ratio of the wave length in vacuo to the wave length in the substance, and $\mu - 1$ expresses the ratio of the retardation to the length of the retarded wave, or, in other words, the fraction of its own retarded length by which a wave is shortened in passing through a transparent substance.

It seems probable, *a priori*, that the study of refractive indices may give valuable information with regard to the constitution of matter. Light is an electro-magnetic disturbance; and even if we do not go so far as to hold, with some physicists, that matter is nothing but a mode of electricity, it is becoming more evident every day that the constitution of the atom is intimately connected with electrical phenomena. It is, therefore, to be expected that the amount of retardation of light in passing

through a medium should be a simple function of the most fundamental properties of the atoms of which it is composed, and that by comparing the retardation when two elements are present but uncombined with that which is caused when combination has taken place, we may even obtain some hint of what takes place in chemical combination.

The subject of physico-chemical research by means of refractive indices may be divided into two main branches: that which deals with solids and liquids, and that which deals with gases and vapours. In this paper it is proposed to give an outline of the more recent developments of the latter division of the subject; but it is impossible to make clear the present position of the problems involved without a brief summary of the work which has been done on the former.

The first observation of importance in this subject was made, as usual, by Newton, who remarked that inflammable substances had high indices of refraction, and thence deduced that the diamond, which had such an index, would prove to be inflammable.

But in Newton's day the science of chemistry did not exist, and we have to pass over a century before we come to the first work, which is still of intrinsic value—the research of Biot and Arago on the refractive indices of gases in 1806. This was followed, in 1826, by the much wider research of Dulong; but in both cases the range of substances examined was comparatively small, and the results were so fragmentary and disconnected that they failed to reveal any orderly connection between the indices of the elements or between the refractivities of compounds and those of their constituents.

With the rapid growth of the science of chemistry during the first half of the nineteenth century, it soon became evident that the study of the refractive indices of liquids, and in some cases of solids, offered a much wider field for research than that of gases, and, accordingly, the attention of experimenters was chiefly directed to this branch of the subject. Between 1860 and 1885 an immense mass of work was done in this department, and results of considerable importance were reached. It is only possible to glance at a few of the most important of these efforts. In 1858 and 1863 Gladstone and Dale published researches on the relation between refractive index and temperature, the refractive indices of mixtures, the comparative refractivities of different members of homologous series and of isomerides, and

the effect of chemical substitution on optical properties. Their general conclusion was that every body has a specific refractive energy composed of the specific refractive energies of its component elements, modified by the manner of combination, and unaffected by change of temperature.

Following up their inquiry, Gladstone gave, in 1870, a very complete list of the "refraction equivalents" of the elements, and in 1885 he published a revised list of these with several important modifications. While Gladstone and Dale were occupied with an extended survey of the refraction equivalents of all the elements, Landolt was engaged on an elaborate examination of those of carbon, hydrogen and oxygen in organic compounds. By tabulating the difference between the numbers representing the refraction equivalents of consecutive members of homologous series, differing in composition by CH_2 , he obtained a mean value for the equivalent of this group, and hence the equivalent of each of the three elements separately. His numbers were: for C, 5.00; for O, 3.00; and for H, 1.30. When these values are used for calculating the refraction equivalents of a large number of carbon compounds of which the refractive index has been measured, the agreement is satisfactory, the difference between calculation and experiment being generally less than 1 per cent.

So good an agreement is only obtained, however, by comparing saturated compounds. To Brühl we are indebted for a series of researches in which it is proved that when two carbon atoms in a molecule are connected by a double bond, the refraction equivalent of the compound is increased by, approximately, 2, or two-fifths of the total effect produced by one atom of carbon. If the number of double bonds is more than one, the equivalent is proportionately larger. If the bond, instead of being double, is triple, the increase is about 1.8. In the results of these researches we seem to perceive the outlines of a fruitful field of work, which needed only patience and labour to yield a plentiful harvest. Yet the outcome of further investigation has been very disappointing. They certainly established the fundamental proposition that, to a first approximation, the power of retarding light is inherent in the atoms of the elements, and gave good grounds for believing that it is not materially changed when they pass from one combination to another. And they sufficed to prove, when Mendeléef's law came to be con-

sidered, that refractivity is a periodic function of the atomic weights of the elements. But the more they are studied the more clearly it appears that Gladstone's figures are not capable of elucidating much further the relation between the constitution of the different atoms, or the nature of chemical combination. The data from which his conclusions are drawn differ widely among themselves, and the figure which he chooses as a constant is often the mean of values which differ by 10 and 12 per cent. Thus his value for potassium, which is the basis of his values for the metals, is the mean of the numbers 7.9, 8.0, 8.2, 8.3, 8.1, 7.8, 7.7, 7.5. Subsequent experiment has shown that whatever difference exists between the refractivity of a compound and the sum of the refractivities of its constituents seldom exceeds 10 per cent. of the whole, and hence values for the elements which may contain an error as large as the whole difference which is being investigated are comparatively worthless.

The more carefully the figures are examined the more numerous are the discrepancies which have come to light, and the subsidiary hypotheses which have to be made in order to reconcile them. Brühl himself showed that when oxygen is united to carbon its refractive effect is greater than when it is combined with two different elements. Conrady confirmed the existence of this distinction, and added a new one, that of oxygen in the position which it occupies in the ethers. Gladstone found that for weak acids the value for hydrogen must be taken as 1.4, while for those which are much ionised he gives 3.4 to 3.5. Le Blanc found two distinct values for chlorine. Wiedemann found two for sulphur. Nasini found a great variation in the results he obtained. The refraction equivalents of nitrogen are very anomalous.

More examples might be given, but it would be superfluous. It is evident that the conception of refraction equivalents and the additive law is only a first approximation to the truth. So long as we direct our attention to compounds which are made up of two or three elements, combined in the same way, fairly concordant results can be obtained: but even these are hardly to be trusted beyond two significant figures. When we apply the additive rule to the elements in general, it is only by courtesy it can be called a success.

The reason of this failure is not far to seek. The true refractive constant of an element is, evidently, the retardation

caused by the free gaseous atom. The forces which compel atoms of different elements to combine and to assume the liquid or solid states, whatever they may be, are evidently very powerful, and it is unreasonable to expect that causes which can modify other attributes of matter in ways and to an extent which we are unable to predict should have no effect, or always the same effect, on its power to retard light.

It is then to the study of gaseous refractive indices that it is necessary to turn in the hope of obtaining really accurate information with regard to the optical properties of matter. In this field the prospects of success *a priori* are all favourable. We know that a gas consists of discrete particles, moving about with velocities which are small relatively to the velocity of light, and separated by distances which are great in relation to their size. The time spent in traversing the free path is long compared with that spent in collisions. We may, therefore, expect that the intrinsic power of each atom or molecule to retard light will be freely exerted without modification by the forces of neighbouring atoms, and that the measurement will represent some fundamental attribute of matter in an intelligible form. At the same time the simplicity of the gas laws greatly facilitates the reduction of the observed index to standard conditions.

The causes which have retarded the expansion of our knowledge in this direction have been the lack of materials and the experimental difficulties. Very few elements and comparatively few compounds are gaseous at ordinary temperatures, and these were quickly disposed of by the early pioneers, Biot and Dulong. It was not till 1861 that Le Roux made a gallant attempt to determine the indices of iodine, mercury, sulphur, arsenic, and phosphorus at high temperatures. He was succeeded by Mascart, who in 1878 greatly enlarged our data by observations on a large number of vapours of liquids boiling below 100°.

Other researches of the same nature followed, and more recently means have been found to overcome the experimental difficulties and to measure indices at temperatures as high as 800° C. with what is believed to be a fair amount of accuracy.

Before turning to an examination of the results obtained, it will be convenient to give a very brief sketch of the methods

employed. When a hollow prism of glass, the faces of which are plane and parallel, is filled with air at the same temperature and pressure as that by which it is surrounded, the light which passes through the prism suffers no deflection. But if some other gas is substituted for the air, the light is refracted, and by measuring the angle of the prism and the temperature and the pressure of the gas its index can be calculated relatively to that of air. The deflection is, however, very small. Thus, in a prism of angle 143° , which is about as large as is convenient (at higher angles it is difficult to obtain glass sufficiently well worked to give a clear image), an atmosphere of air would cause a deflection of $6'$; and in order to measure the index to one part in a thousand it would be necessary to read the position of the object through the prism to one-third of a second of arc.

The difficulties of the prism method depend on this fact. If the faces of the prism are not absolutely plane and parallel, there is a deviation due to this cause, which varies with the temperature of the glass. It is also necessary to know the temperature, pressure, and refractive index of the air which surrounds the prism accurately, and the stability of the optical arrangements must be of a high order. Owing to these difficulties the prism method, which was used by Biot and Arago, by Dulong and by Le Roux, was abandoned by Mascart after trial, and has not been used recently in any great research, except that of Kayser and Runge on the dispersion of air in 1893. In its place a method founded on the interference of light waves has been adopted. There are many varieties of apparatus, but they are nearly all based on the same principle. A beam of monochromatic light is divided into two portions, which are subsequently reunited in the field of a telescope. If the path of one of these two beams is retarded relatively to the other by half a wave length, the two beams interfere and no light is seen. When the retardation is an exact multiple of a wave length, the two reinforce one another and the brightness of the field is increased. If, then, a source of monochromatic light is viewed through such an arrangement, the result is that in the field of view of the telescope is seen a number of bright and dark bands which move across the field as the path of one of the interfering beams is continuously retarded, at the rate of one band for each additional wave length of retardation. In practice, the two beams pass through different

tubes, one of which contains the gas under examination. When the pressure of the gas in one tube is changed, the bands which pass the cross-wire in the telescope are counted, and the refractive index of the gas is found from the equation

$$\mu - 1 = \frac{N\lambda}{L} \frac{760}{P} \frac{t}{273}$$

where N is the number of bands, λ the wave length, L the length of the tube, t the absolute temperature, and P the change of pressure in millimetres.

This method is capable of extreme accuracy. Apart from the impurity of the gas, which is generally much the largest source of error, the limits of its effectiveness are to be found in the difficulty of reading the bands to less than a tenth, in counting a large number of bands without mistake, and in the drift of the zero point during the experiment owing to unequal changes of temperature in different parts of the apparatus. And of these the last is the most important. In order to ascertain the density of the substance under examination, various methods are employed. With permanent gases which do not attack the materials of the apparatus the temperature and pressure are directly observed. If the gas attacks mercury, it is led into the refractometer tube at atmospheric pressure, and the bands read as it gradually displaces the air.

With vapours the temperature and pressure are sometimes read, and the density calculated from previously ascertained data. But this method is not accurate, and it is much better to have a density bulb in connection with the refractometer, and to take the density directly. With solids and liquids which require high temperatures to vaporise, such as mercury, the most fruitful method has been to introduce a weighed quantity into the refractometer tube, which is evacuated and sealed off. The temperature is then raised till the whole of the substance is vaporised and the density is calculated from the weight of the substance present and the volume of the tube. Hitherto this plan has not been much used for compounds, and a rich harvest of important results would probably reward any one who would take up this branch and work it out methodically.

There are three great problems upon which it seems probable that the study of refractive indices will throw light: the relative

indices of the elements, which may tell us how they differ from each other; the comparison of the indices of compounds with that of their constituents, which may throw light on the nature of chemical combination; and the dispersion of light by elements or compounds, which is confidently looked to for a clue to the actual configuration of the atom, and in particular to the part the electron plays in its composition.

Of these the first is the most fundamental, and it was therefore natural that the early workers should begin by examining those elements which lay nearest their hands. Biot¹ and Arago measured the refractivities of hydrogen, oxygen, and nitrogen. Dulong repeated their work and added chlorine. But here all further progress was checked by want of material. Bromine had only just been discovered, and all the other elements required so large a rise of temperature in order to vaporise them that they were outside the sphere of practical work at the time. Nor did the results obtained in the case of these elements seem to foreshadow the existence of any simple relation between the indices of the elements, such as might have encouraged the pioneers to further exertions. The refractivity² of hydrogen is 139, of oxygen 270, and of nitrogen 297. At that time Newton's emission theory of light held the field, and it was sought to connect the acceleration of light as it was then thought to be with the density of the medium. But neither when these figures were regarded simply, in which case they represented the relative retardations of equal numbers of atoms, nor when they were divided by the atomic weights, when they were proportional to the relative retardations caused by equal weights of different elements, was any clear rule visible. By the first calculation hydrogen retards light less than the others; but for unit weight its refractive power is the greatest known. Nitrogen, with a lower atomic weight than oxygen, retards light slightly more, and chlorine, with higher atomic weight, had a refractivity of 768, very much above the others.

¹ Biot calculated his results for air to ten places of decimals, though his experimental results only agree to six, and when this was pointed out he replied sarcastically that if the first figures were wrong, perhaps the last would be right.

² The term "refractivity" is used to mean $(\mu - 1) 10^8$ and is proportional to the absolute amount of retardation of light passing through a gas of standard density. Thus for air at 0° and 760 mm., $\mu = 1.000292$ and the refractivity is 292. In other words, light is retarded nearly $\frac{1}{100000}$ of its velocity in vacuo while passing through air under these conditions.

It was evident that nothing could be made out of such scanty material.

In 1861 Le Roux attempted to enlarge the available data by attacking iodine, sulphur, phosphorus, arsenic, and mercury by heating them in a hollow glass prism; but as his figures have not been confirmed by subsequent investigation, they need not be given.

In 1874 M. Mascart added bromine to the list, finding for it the high value of 1,125, and in 1877 M. Hurion published two values for the index of iodine, finding for the red rays a refractivity of 2,050 and for the violet 1,920, numbers which derive an additional interest from the fact that they showed, for the first time, a case of anomalous dispersion in a gas.

Thus at the close of the nineteenth century we were in possession of the gaseous refractive indices of only six elements, hydrogen, nitrogen, oxygen, chlorine, bromine, and iodine. The first three showed little or no connection, while of the last three no more definite statement could be made than that the refractivity increased concurrently with the atomic weight.

With the commencement of the new century a considerable advance was made. In 1901 Ramsay and Travers published the results of their investigations on the five inert gases of the atmosphere. These elements—helium, neon, argon, krypton, and xenon—are of peculiar importance in the study of optical constants. They are all gases at the ordinary temperature, and they are all members of the same chemical group, of zero valency. In all, too, the molecule is monatomic. It was to be hoped that, if any discoverable relation existed between the refractivities of the elements, it would show itself in this group.

The result showed that this expectation was not unfounded. The refractivities of these five gases proved to be almost exactly in the ratios of 1, 2, 8, 12, 20.

Since the original measurements were made four out of the five indices have been redetermined with larger quantities of gas and with better methods of purification, and the approximation to simple integral ratios is now even closer, whether we consider the index for infinite waves or that for the D line, as the table below shows.

The question immediately suggested itself whether any other group of elements showed similar relations; and, fortunately,

the halogens chlorine, bromine, and iodine were available for comparison. On examining them it was at once apparent that the same ratio of 8, 12, and 20 holds between their refractivities as between argon, krypton, and xenon; the discrepancy being only 2·3 per cent. in the case of bromine and nil in that of iodine for the violet, though owing to the great anomalous dispersion the red shows a divergence of 6·4 per cent. If we turn to the neighbouring group of the alkalies we are unable to obtain any figures for their gaseous indices, the experimental difficulties having hitherto proved insuperable. But if we fall back on the refraction equivalents of Gladstone, calculated from liquids, we here again find a most remarkable correspondence, the numbers for potassium, rubidium, and cæsium being 7·85, 12·1, and 19·2, which are sufficiently near the ratios of 2, 3, and 5 to show that in all probability these figures would be found if we could obtain the gaseous indices for infinite waves. It was plain that in the face of so curious a set of coincidences an effort should be made to extend the range of available data to other elements, and some years ago a research with this object was undertaken. The results have been more gratifying than could have been anticipated. It was found that the index of fluorine, which had been supposed to be extraordinarily low, if indeed it was not less than unity, was just one quarter that of chlorine, and those of sulphur and phosphorus were almost exactly four times those of oxygen and nitrogen respectively. When, however, the research was extended to the higher members of the polyvalent elements, the simplicity of the ratios begins to be obscured. The refractivities of selenium and tellurium fall short of the numbers expected by analogy by 4·7 and 8·7 per cent., and that of arsenic by 17·4 per cent.; while in the case of zinc, cadmium, and mercury no simple relation seems to exist; mercury, with the highest atomic weight, having a lower refractivity than zinc or cadmium.

The following is a list of all the elements whose gaseous indices have now been determined, some with more and some with less accuracy, and in Table II. (on p. 285) are shown, in numbers rounded on the assumption that the rule of simple integers holds, the relative gaseous refractivities of the elements so far as they are known. The position of the elements whose gaseous index is unknown is added in order to make the table more intelligible.

TABLE I

Element	Refractive Index	Refractivity ($\mu - 1$) 10^6		Empirical Ratios.	Divergence of experiment from col. 5 per cent.		Observers.
		$\lambda = \infty$	$\lambda = 5893$		$\lambda = \infty$	$\lambda = 5893$	
	$\mu - 1 = a \left(1 + \frac{b}{\lambda^2} \right)$						
Hydrogen .	$\cdot 0001358 \left(1 + \frac{6 \cdot 67}{10^{11} \lambda^2} \right)$	135'8	138'4				Scheel
Helium .	$\cdot 00006956 \left(1 + \frac{2 \cdot 2}{\lambda^2 10^{11}} \right)$	69'56	70'00	1	Standard		W. Burton
Neon .	$\cdot 1'0001374$		137'4	2		- 1'9	Ramsay and Travers
Argon .	$\cdot 0005584 \left(1 + \frac{5 \cdot 6}{\lambda^2 10^{11}} \right)$	558'4	567'4	8	+ '3	+ 1'3	W. Burton
Krypton .	$\cdot 0008378 \left(1 + \frac{6 \cdot 97}{\lambda^2 10^{11}} \right)$	837'8	854'6	12	+ '4	+ 1'6	C. and M. Cuthbertson
Xenon .	$\cdot 00013646 \left(1 + \frac{10 \cdot 14}{\lambda^2 10^{11}} \right)$	1364'6	1404'	20	- 1'9	+ '3	" "
Fluorine	$\cdot 1'000195$		195	2		+ 1'5	Cuthbertson and Prideaux
Chlorine	$\cdot 1'000768$		768	8	Standard		Mascart
Bromine	$\cdot 1'001125$		1125	12		- 2'3	"
Iodine {	$\cdot 1'00192$ violet			20 {		0	Hurion
	$\cdot 1'00205$ red					+ 6'4	
Oxygen .	$\cdot 0002663 \left(1 + \frac{5 \cdot 07}{\lambda^2 10^{11}} \right)$	266'3	270'2	2	Standard		Mascart
Sulphur .	$\cdot 0010457 \left(1 + \frac{21 \cdot 2}{\lambda^2 10^{11}} \right)$	1046'	1111	8	- 1'8	+ 2'2	Cuthbertson and Metcalfe
Selenium	$\cdot 1'001565$		1565	12		- 4'	" "
Tellurium	$\cdot 1'002495$		2495	20		- 8'	" "
Nitrogen .	$\cdot 00029061 \left(1 + \frac{7 \cdot 7}{\lambda^2 10^{11}} \right)$	290'6	297'1	2	Standard		Scheel
Phosphorus	$\cdot 001162 \left(1 + \frac{15 \cdot 3}{\lambda^2 10^{11}} \right)$	1162'	1212'	8	0	+ 2'03	Cuthbertson and Metcalfe
Arsenic	$\cdot 1'001552$		1552'	12		- 17'	" "
Zinc .	$\cdot 1'002050$		2050'	?			" "
Cadmium	$\cdot 1'002675$		2675'	?			" "
Mercury .	$\cdot 001755 \left(1 + \frac{22 \cdot 65}{\lambda^2 10^{11}} \right)$	1755'	1866'	?			" "

The broad facts of this table are at the same time very simple and very puzzling. In no less than five different groups of the elements we find that at least in one case and sometimes in more than one the refractivities are in the ratio of integers so simple as 1, 2, 8, 12, and 20, of which the last three can be reduced to 2, 3, and 5.

If we except the case of mercury, or challenge its admission to the group which contains zinc and cadmium, the rule is that in each group the refractivity increases as the atomic weight increases, though not regularly. But for the case of helium it could be said that the refractivity increases much faster than the atomic weight, *i.e.* in the ratios of 1 to 10, while the atomic weight increases in the ratio of 2 to 13; but between

helium and neon the reverse is the case, and the atomic weight increases more than twice as fast as the refractivity. But the most extraordinary fact is the great jump from the row which contains neon to that which contains argon, when the refractivity increases fourfold; coupled with the fact that this relation is exactly repeated in the case of nitrogen and phosphorus, oxygen and sulphur, fluorine and chlorine. These facts point to the disconcerting conclusion that no formula can exist which will connect refractivities directly with atomic weights, for while the refractivities of adjacent numbers of two neighbouring groups are in the same ratio, their atomic weights are not.

The next point, which is of great importance, is that refractivity is evidently connected closely with valency. An examination of the horizontal row containing nitrogen, oxygen, fluorine, and neon shows that the last named, with the highest atomic weight, 20, has much the lowest refractivity. Next comes the monovalent fluorine, then the divalent oxygen, and finally trivalent nitrogen with the lowest atomic weight and the highest refractivity. But this is not all. If we could measure the refractivity of gaseous carbon, it would, on the same scale, lie somewhere between 330 and 360. It thus appears that increase of valency is accompanied by increase of refractivity. In what proportion the increase occurs it is not possible to say at present, for the atomic weight is not constant, and the relation between refractivity and atomic weight is not known; in other words, we have no data for calculating what the refractivities of N, O, and F would be if they all had an atomic weight of 20. It is clear, however, that a large portion of the refractivity is independent of the valency; otherwise the non-valent elements would not retard light at all.

Gladstone thought that the product of specific refractivity by the square root of the chemical equivalent was constant, but the figures do not support this view.

Perhaps some light may be shed on the problem by a study of the table of the elements given above.

The elements are there shown in the order of their atomic weights, as in Mendeléef's table; but the distribution of the rows is so arranged that the elements whose refractivities are in the ratio of 2, 3, 5 in neighbouring groups are adjacent. The result is to exhibit the table in the shape of a pyramid rather

TABLE II.

[illegible]

than in that of a rectangle. This scheme has certain very suggestive features. It fits in very well with the periodicity of the atomic volumes, and satisfies most of the requirements of such a table. Above all, it lends itself to the consideration of those fascinating speculations on the origin of the elements which are absorbing the attention of so many eminent physicists and chemists at the present time. If atoms are made up of ponderable matter and relatively imponderable electrons, as the phenomena of radio-activity indicate; if successive elements are built up, as Professor J. J. Thomson suggests, by the addition of electrons forming rings round a central mass, each more numerous than the last; if there is a close connection between valency and the number of electrons per atom, or the number of electrons after subtracting the number required to form a perfect ring; if successive powers of two are such numbers; and if refractivity is closely connected with the number of electrons and the forces under which they vibrate, then we can dimly perceive that the pyramidal shape of the table (indicating a gradual increase in the amount of matter required to make the wheel of evolution come full circle as the atomic weight increases), the discontinuity in the refractivities, and their connection with valency are all intimately connected. What the configuration of the atom must be in order to satisfy experimental facts is the fascinating problem which continues to tantalise us. It can hardly be doubted that if we could obtain the indices of all the elements correctly,

much light would be thrown on the nature of the atom. Unfortunately there is not much hope that many more indices will be brought within our knowledge directly, as the vapour pressures of the elements are too low at temperatures at which even a quartz refractometer tube softens. The alkali elements still remain to be the prize of whoever will find a material by which they are not attacked at high temperature. Iron tubes closed with plates of white sapphire would probably do ; but the difficulties are considerable. The less volatile elements zinc, cadmium, arsenic, selenium, and tellurium should certainly be repeated with greater accuracy ; and chlorine, bromine, and iodine should be remeasured. It is possible that by a special effort gallium, indium, germanium, tin, lead, and bismuth might be secured.

The investigation of the indices of gaseous compounds, with the object of discovering the connection between the refractivity of a compound and that of its constituents, is a branch of the inquiry which promises to throw a great deal of light on the nature of chemical combination. Refractivity, as has been pointed out above, is probably in some manner intimately connected with the number of electrons which help to constitute the atom, and, as will be shown later, with the period of their free motions with reference to the atom. In proportion as the frequency of the light waves which impinge on the atom approaches that frequency which is natural to the electrons, a large part of the energy of light is absorbed in setting in motion or in increasing the motions of the electrons. In so far as the medium absorbs light, the loss of energy falls upon the amplitude of the vibrations. In so far as it is transparent, it falls upon the wave length, or in other words on the refractivity. If it were found that the refractivity of a compound were the sum of the refractivities of its constituents, the inference would be that each atom preserved in the compound its whole individuality and freedom of movement unchanged by the proximity of that with which it was combined. But if it could be shown that the refractivity of the compound is materially different from this, it follows that to this extent combination has altered the factors on which the refractivity of the atom depends, by altering the position of the electrons, or the forces which control their movements. It cannot be doubted that these forces are complex ; and it is, therefore, to be expected that only

by collecting a large number of observations, and by carefully comparing the observed changes in refractivity, are we likely to make any progress in unravelling these difficult problems. But though the importance of this branch of the subject has been insisted on by every investigator who has approached it, several circumstances have conspired to prevent results of interest being obtained from its study. Not only is the number of gaseous compounds comparatively small, but the number of elements whose indices were known was till recently so restricted as to limit still further the choice of compounds on which the comparison could be made; and, to crown all, the first compounds whose indices were measured now prove to have been the most anomalous in the whole range of physics. It is not surprising, therefore, that research in this direction was not pursued very eagerly. Biot and Arago could only make use of water and ammonia; and the errors in their work, due to the primitive condition of both chemistry and physics at the time, render the results of little interest except historically. They concluded that the refractivities of the compounds are the sum of the refractivities of their constituents, at least in compounds such as ammonia, "where the condensation is not very great." They are thus responsible for formulating the additive law for gaseous indices.

Dulong (1826) published the results of a systematic research on twenty-two gases and vapours. By assuming a certain probable value for the refractive power of carbon, he was able to utilise, in a fashion, the figures for compounds which contained this element, and he concluded that the refractive powers of gases have no relation to their densities, and have nothing to do with the heat of combination, and that, as a rule, the refractive power of a compound is greater than that of its components, when the compound is neutral or alkaline, and less when acid. He added that the difference of refractivity appears to depend on the electrical state proper to the molecules of each sort of matter, and that on the undulatory theory they appear to retard light in proportion to their positivity. To these conclusions he was probably led by the values he found for N_2O , NO , and NH_3 which, as will be shown later, are anomalous. But it must be admitted that it was a very shrewd guess which, in 1826, connected refractivity with electrical state.

Ketteler and Lorenz, who were mainly interested in dispersion, re-determined the indices of some of the substances dealt with by Dulong; but the next important landmark in the history of the subject does not occur till 1874-8, when Mascart¹ published his researches on a large number of compounds.

These papers form the largest store of facts extant. But for the purpose of an inquiry into the relation between the refractivities of the elements and their constituents, the value of his work is discounted by three circumstances. The range of compounds which he could attack was limited by the form of his apparatus, which did not permit experiments at a higher temperature than 37° C. His results were in some cases calculated to express what the refractive index of the compound would be at 0° and 760, irrespective of the density; in others they were simply compared with that of air at 12° C., and they cannot easily be reduced to a form strictly comparable with that of the indices of the permanent gases. Further, in a large number of cases, the compounds which he examined contained carbon, and, as the gaseous refractivity of this element has not been measured, all these figures are unavailable for the purposes of the present inquiry. Indeed, at the end of his research he was in possession of only six compounds, of which the refractivities of all the components were known to him. These were NH_3 , NO , N_2O , HCl , HBr , H_2O .

It was very unfortunate that, of these six, three were nitrogen compounds, which subsequent comparison proves to be anomalous. This circumstance misled Mascart to the conclusion that the refraction of a compound is in general more than that of its components. He added that it was not possible to calculate the refractivity of a gas from its components.

This view seems to be that which is generally held at the present time. Ostwald, in an able review of the subject, goes so far as to state that there is no relation to be traced between the indices of the elementary gases and their compounds, and Brühl, in a well-known paper, definitely concludes that in gaseous compounds refractivity is certainly not an additive quality, but one which is constitutive. Since Brühl's paper was published not much work appears to have been done on this

¹ Mascart, *Ann. de l'Ecole Normale*, sup. (2), t. vi. p. 9, 1877; *C.R.*, t. lxxxvi. pp. 321, 1182 (1878).

subject, despite the fact that so important a law as that which is here involved is still unguessed. Fortunately, however, the determination of the indices of phosphorus and sulphur has added a few compounds to the slender list of available data, and though we are not yet far advanced beyond the negative conclusions of Mascart and Brühl, it is certainly time that the information we possess should be reviewed and examined afresh in the light of work which has been done in other directions. The following is a list of all the compounds whose gaseous index, and the gaseous index of both whose constituents, have been determined. Where observers differ, and are of approximately equal authority, more than one value is given. The third column gives the actual refractivity observed (in general for yellow light). The fourth column shows the sum of the refractivities of the constituents. The fifth column shows the difference between the third and fourth column, and it is reckoned positive when the sum of the retardations of the constituents is less than that of the compound. The sixth column shows the percentage of the change to the additive value.

TABLE III

Refractivities of compounds of which the refractivities of all the components are known

Compound	Light	Refractivity observed	Refractivity calculated by addition	Difference per cent.	Observer
NH ₃	D	377	148 + 208 = 356	+ 5.9	Mascart
N ₂ O	"	373	—	+ 4.8	Lorentz
NO	"	515	297 + 135 = 432	+ 19.3	Mascart
HCl	"	297	148 + 135 = 283	+ 5.0	"
HBr	"	444	69 + 384 = 453	+ 1.98	"
HI	"	570	69 + 562 = 631	+ 9.77	"
HI	Red violet	906	69 + 1025 or 960	{ -18.4 } { -12 }	Hurion
H ₂ O	D	257	138 + 135 = 273	+ 5.8	Mascart
H ₂ S	"	250	—	+ 8.5	Lorentz
H ₂ S	White	641	138 + 555 = 693	+ 7.5	Dulong
SO ₂	D	619	—	+ 10.7	Mascart
SO ₂	"	660	555 + 270 = 825	+ 20	G. W. Walker
SO ₂	D	737	555 + 405 = 960	+ 23	Cuthbertson and Metcalfe
SF ₆	"	783	555 + 585 = 1140	+ 32	" "
SeF ₆	"	895	782 + 585 = 1367	+ 34.5	" "
TeF ₆	"	991	1247 + 585 = 1832	+ 45	" "
TeCl ₄	"	2600	1247 + 1536 = 2782	+ 6.6	Prideaux and Metcalfe
PH ₃	White	786	603 + 208 = 811	+ 3.8	Dulong
PCl ₃	D	1730	603 + 1152 = 1755	+ 1.4	Mascart

It will be noticed that the list is extraordinarily meagre considering the volume of work which has been done on kindred subjects. It is true that in addition a very long list could be made of the carbon compounds whose gaseous index has been determined. Most of these are, however, comparatively useless for the present purposes, since they consist of large complicated molecules. But the following, which are among the simplest, and involve only one unknown refractivity, that of carbon, may be selected :

TABLE IV
Refractivities of some carbon compounds

Compound	Light	Refractivity	Refractivities of known components	Refractivity of carbon by difference	Observer
CH ₄ . . .	D	441	278	163	Mascart
CH ₃ F . . .		449	305	144	Cuthbertson
C ₂ H ₂ . . .		1701	831	174	Mascart
CH ₂ Cl . . .		865	592	273	"
CHCl ₃ . . .		1455	1221	234	"
CCl ₄ . . .		1768	1536	232	"
CO ₂ . . .		449·8	270	179·8	Perreau
CO . . .		334	135	199·5	Mascart
C ₂ H ₂ . . .		606	139	234	"
C ₆ H ₆ . . .		1812	417	233	"
" . . .		1765	—	215	Prytz
C ₂ H ₄ . . .		719	278	220	Mascart
" . . .		674	—	198	Prytz
CH ₃ OH . . .		552	413	139	"
" . . .		619	—	206	Mascart
CS ₂ . . .		1476	1111	365	"

A study of these figures confirms quite definitely the conclusions of Brühl that the law, whatever it be, is not additive. In not a single instance is the refractivity of the compound equal to the sum of its constituents, and the divergence is in most cases far greater than can be explained by errors in experiment. Thus, for example, the refractivities of oxygen and hydrogen are certainly known to less than one part in a thousand, but the refractivity of water vapour differs from the additive value by about 5 per cent.¹ Nor can it be affirmed that an element has one refractive power when free and another which, though different, is constant in combination. For if we

¹ It is much to be regretted that the experimental values for water differ so widely ; a new determination is urgently required.

choose four compounds involving three elements, and, treating the numbers and symbols as equations, solve for the refractivities of the elements, we obtain inconsistent results.¹

This result is of fundamental importance, and shows that the law connecting refractivities of compounds and elements is still far from being found. In seeking for it, we observe first that, as a rule, the refractivity of a compound is less than that of its constituents by an amount which varies from 1 to 45 per cent. To this rule there are, however, three certain exceptions, and several which are probable. The nitrogen compounds, N_2O , NO , and NH_3 , all show an increase of refractivity; and if the sum of the refractivities of their constituents, and of the refractivity of gaseous nitric acid, be calculated—as it can be with a fair approach to accuracy—from the liquid, it is found to follow the same direction. Another probable exception is CS_2 , the index of which is so high that the value found for C by difference is almost certainly greater than that of an uncombined carbon alone. These exceptions suggested to Dulong that the change is connected with thermo-chemical effects, since N_2O and NO and CS_2 are endothermic. But the refractivity of NH_3 , which ought on this hypothesis to show a decrease, and really shows an increase, negatives the supposition.

With these exceptions, which are worthy of the most careful study, the general rule for elements is to lose refractive power on combination; and if we examine the figures given in Tables III. and IV. in detail, some very interesting tendencies can be discovered. The most important series, because it is the simplest, is that of the halogen acids. In HCl we have a reduction of 1.98 per cent. on combination, in HBr of 9.77, and in HI of 18.4 per cent. or 12 per cent. according as we use the accepted value of the refractivity for the red or violet. This progression is sufficiently remarkable. But the method of expressing the change of the sum of two very unequal quantities as a percentage of the whole is evidently unsatisfactory. If the figures are regarded in a different way, we obtain a still more interesting result. It may be said that the refractivity of a

¹ For example:—

$$2H + O = 250$$

$$2H + S = 619$$

$$S + 2O = 660$$

whence $S = 466$; $O = 97$; $H = 76$; values which do not fit the equation $S + 3O = 737$.

molecule of HBr is only eight points higher than that of one atom of bromine, so that if the whole change in refractivity could be debited to the hydrogen atom, it could be said that eight-ninths of its refractivity have disappeared. But, turning to HI , a greater surprise is in store. The refractivity of a molecule of hydriodic acid is actually less than that of an atom of iodine alone. To what cause is this due? The answer would perhaps supply us with the clue to the nature of chemical combination. On the assumption that refractivity is due to the action of the electrons which enter into the composition of an atom, it is a plausible guess that in combination one or more electrons jump across from one atom to the other, and that in its new position it is in a field of force where it does not exert so great a retarding effect. Such a theory has an attractive similarity to the conceptions of electrolytic theory. It has actually been put forward as a possibility by Sir William Ramsay and others, and it is worthy of further examination.

The tendency to the reduction of refractivity shown in the cases above mentioned is not without parallel. In 1906 the refractivities of the hexafluorides of sulphur, selenium, and tellurium were determined in the gaseous state. The purity of the compounds is guaranteed by the tests to which Dr. E. B. R. Prideaux¹ subjected them.

The results show a large reduction of refractivity on combination in all three cases. A molecule of SF_6 shows a reduction equal to more than half the refractivity of a sulphur atom. A molecule of SeF_6 retards light very little more than an atom of selenium, while a molecule of TeF_6 retards it by a good deal less than an atom of tellurium alone. In this case retarding power equal to the six fluorine atoms and one quarter of the tellurium atom had disappeared. Though no explanation of these facts can be offered, it is suggestive to reflect that the great chemical activity of fluorine is accompanied by the power of forming compounds of low refractivity. One very interesting conclusion can be drawn from the two series. The case of TeF_6 shows that on the hypothesis that the six fluorine atoms lose refractive power by losing an electron each, the tellurium atom must also lose refractivity in gaining six electrons, and any theory which seeks to explain the nature of refractivity must take that

¹ "Some Reactions and New Compounds of Fluorine," by E. B. R. Prideaux (*Trans. Chem. Soc.* 1906, vol. 89, p. 316).

important fact into account. The case of the sulphur compounds, H_2S , SO_2 , and SO_3 , is very interesting. The first of these loses nearly 11 per cent. of its refractive power on combination, the second 20 per cent., and the third 23 per cent.; and it is significant that the refractivity of gaseous sulphuric acid, calculated from the liquid, is also extremely low. Here we have cases in which, both when sulphur is negative, as in H_2S , and when it is positive, as in SO_2 , and SO_3 and SF_6 , a large reduction of refractivity takes place as a result of combination. The phosphorus compounds given in the table are also interesting. Though there is a reduction of refractivity on combination, it is so small compared with the analogous figures for sulphur that it suggests a tendency on the part of phosphorus to form compounds of unduly high refractivity like nitrogen.

It would take too long to discuss the interesting features presented by even the slender list of carbon compounds given in Table IV. Our ignorance of the refractivity of free carbon detracts from their value, and all that can be done is to draw some rough conclusions by calculating what the value for carbon would be if the additive law held. From column 5 we see that the average value for one atom of carbon is about 170, which is considerably higher than that of one atom of nitrogen, 147. Marked differences arise, however, in different compounds. When a carbon atom is united with four hydrogens, whose refractivity is low, the additive value comes out low (163), as if the carbon had somehow lost some of its refractive power. When the four hydrogen atoms are replaced by four chlorine atoms, the refractivity of which is higher than that of carbon, the additive value comes out higher (232), and still higher in CHCl (273). Unless this anomaly turns out to be due to the adoption of different standards of density or to experimental errors, it is an important clue.

One of the most striking facts which can be deduced from the carbon compounds is that when a compound is unsaturated, as in CO , C_2H_2 , the refractivity of the compound is greater than it would have been otherwise. This has been very fully proved in the case of liquids, and Brühl has assigned definite increases to the refractivity of the carbon and oxygen atoms under these circumstances. The number of gaseous compounds of this nature whose index has been determined with accuracy is not sufficient to enable us to confirm Brühl's law; but existing

figures for gases do not tend to discredit it. The tendency certainly exists. It is most marked in the unsaturated nitrogen compounds, but the presence of the same tendency in NH_3 and HNO_3 shows that unsaturation is not the only cause.

There can be no doubt that, if some good explanation could be found for this group of facts, we should have a valuable means of attacking the problem of refractivity. But the facts are puzzling, and until a large quantity of fresh data are available for discussion it is not likely that the riddle will be guessed.

The third problem on which it is probable that the study of refractive indices may throw some light is the nature of the atom itself and the part which electrons play in the structure. It is in relation to this question that the study of dispersion assumes importance. So little work has, however, been done on comparative dispersions that it would be useless to devote much space to an examination of a subject by no means simple, and a very brief and elementary account of the position must suffice.

In a vacuum light of all wave lengths travels with the same velocity. In material media not only are all wave lengths retarded, but the retardation of the long red waves is generally about 1 to 2 per cent. less than that of the short violet waves. In a few cases, however, the retardation of the shorter waves is less than that of the longer, and the dispersion is then said to be anomalous. To explain and correlate these facts is the problem attacked in the study of dispersion. The theory which finds most approval at the present day, and which is strongly supported by many lines of evidence, is that the retardation of light in passing through a material medium is caused by the expenditure of energy in setting and keeping in motion the electrons which form part of the atom, or, possibly, the atom itself. Whatever the configuration of the parts of the atom may be, they are so balanced by the forces acting upon them that they have a natural period of vibration of their own. If upon a system of such atoms, forming a gas, a beam of white light, which contains waves of all wave lengths, is allowed to fall, the waves whose frequency is very different from the natural frequency of the parts of the atom are little affected by the vibrations of those parts, and lose little of their energy and of their velocity. But as the period of the light in question approaches that of the parts of the atom, its energy is more and

more available for setting them in motion, and the consequent loss of energy and velocity is greater.

The best-known theory on the subject is that due to Lorentz, which states that, on the assumption that all the electrons involved in the atom are of the same sort, *i.e.* have the same period of vibration, the refractivity is directly proportional to the number of electrons, and, inversely, to the difference between the squares of the frequency of the light in question and the free period of the electrons. If there are several classes of electrons, with different free periods, then the total retardation is the sum of the retardations imposed by all. Such a theory, even in the simple form here stated, is discouraging to the experimentalist. For aught we can tell every atom may contain a thousand or a million electrons, no two of which have the same natural period of vibration, and the task of evaluating their total effect on light and comparing it with experimental results would be impossible. All that we can do is to hope that the truth is simpler than this, and that by measuring the dispersion of different elements and comparing those of the same chemical group, we may obtain valuable information with regard to the relative number of electrons they contain.

At present the results of experiments are not sufficiently numerous to enable us to draw any conclusions of value, and more work is greatly to be desired.

Such figures as exist are shown in Table II.¹

The dispersion of hydrogen, nitrogen, oxygen has been measured more than once, but does not appear to exhibit any simple relation. Indeed, it is not to be expected that such a relation should be found at present, for the discrepancies between the results of different observers are so large that the figures are practically worthless. Thus, Mascart himself points out that

It will be noticed that the dispersions are expressed in terms of Cauchy's formula :

$$\mu - 1 = A \left(1 + \frac{B}{\lambda^2} \right)$$

which is, strictly speaking, irreconcilable with that of Lorentz :

$$\mu - 1 = \Sigma \frac{C}{\rho^2 - w^2}$$

where ρ is the free frequency of the electron and w that of the light ; but since the free period is certainly very remote from the visible spectrum the discrepancy is unimportant, and the constant B expresses the existing experimental results for dispersive power with sufficient accuracy.

his values for the dispersion of oxygen and nitrogen are both greater than that of air, so that one of them must be wrong. Dufet, in his collection of optical data, records no attempt to measure the dispersion of chlorine or bromine and only one rough observation on iodine. It is very much to be regretted that these comparatively easy experiments should not be performed. The dispersion of mercury, sulphur, and phosphorus has been measured recently, and in these results we find the first indications of what may, if confirmed, turn out to be a simple relation. The dispersive power of sulphur is found to be about four times that of oxygen, and the dispersive power of phosphorus almost exactly twice that of nitrogen. But the most promising results have been obtained from the inert gases. It is found that, in the four cases in which the dispersions have been measured, if the refractivity for infinite waves is plotted against the value of b in Cauchy's formula the result is a straight line. If this rule should be confirmed in the case of the fifth gas, neon, it may perhaps assist mathematicians in the selection of the hypotheses connected with their theories of dispersion.

There is one further result which is worthy of the attention of mathematical physicists, and is, I believe, the only one of its class at present. The dispersion of SO_2 has been measured by Ketteler, and can be compared with that of sulphur and oxygen. If Ketteler's figures are reduced to the standard density, the three formulæ would then stand thus:

For one atom of sulphur—

$$\mu - 1 = \cdot 000523 \left(1 + \frac{21 \cdot 25}{\lambda^2 \cdot 10^{11}} \right) \quad \text{Cuthbertson and Metcalfe.}$$

For two atoms of oxygen—

$$\mu - 1 = \cdot 0002663 \left(1 + \frac{5 \cdot 07}{\lambda^2 \cdot 10^{11}} \right) \quad \text{Scheel.}$$

For one molecule of SO_2 —

$$\mu - 1 = \cdot 000642 \left(1 + \frac{9 \cdot 6}{\lambda^2 \cdot 10^{11}} \right) \quad \text{Ketteler corrected.}$$

The figure 9·6 is the mean of the two values 9·1 and 10·1 obtained from Ketteler's three values, and is consequently not to be trusted to 10 per cent. Roughly speaking the dispersive power of SO_2 is half that of sulphur and double that of oxygen.

It is much to be desired that similar figures should be obtained for HCl , HBr , HI , H_2S , and many other compounds.

In conclusion it will be convenient to summarise the points which it is the object of this brief review to enforce. The study of the refractive indices of elements and compounds is universally admitted to be of importance in throwing light on the nature of atoms and molecules; but the comparative sterility of the work which has been done on the indices of liquids and solids proves that in these states the causes which tend to obscure the simplicity of result which we hope for are too powerful, and that it is vain to hope for much addition to our knowledge in this direction. It is, therefore, to the study of gaseous indices that attention should be directed. The field of research is here extremely wide, and it is comparatively untrodden ground. The little work that has been done has led to the observation of a series of simple relations which can hardly be believed to be mere coincidences; and which, by their very simplicity, seem to be the outward and visible sign of some fundamental truth, which ought to be pursued. Nor can the absence of any plausible hypothesis be considered a good reason for neglecting to push the inquiry further. The series 1, 2, 8, 12, 20, which connects the refractivities of the inert gases, is anomalous from a mathematical point of view; but the periodic table of the elements itself is not free from similar difficulties, and it is possible that the anomalies of the one may help to explain the anomalies of the other. In the region of the indices of the compounds, such work as has been accomplished has sufficed to prove that the additive law, which till recently held the field, is so far from being true, even as a first approximation, that it must certainly be abandoned. The discovery of the true law can hardly be attempted without the accumulation of a large mass of well-selected data. Hitherto, the work has been of a pioneer character. What is now required is organised research on typical series of the simplest compounds, selected with intelligence. As examples of the existing dearth of material it may be mentioned that no attempt has hitherto been made to find out whether gaseous polymerisation has any effect on refraction, *i.e.* whether two molecules are as effective in refraction when linked together as when separate. A study of NO_2 and N_2O_4 , of acetic acid and of ozone would go far to set this question at rest; but this has not yet been attempted. Again, Dufet records that only two of the hydrocarbons, methane and pentane, have been investigated in the gaseous condition. A long list could be

made of the simple compounds which have not yet been touched.¹

Finally, it is to be confidently hoped that a study of the dispersion both of the elements of compounds and their relation to the dispersion of their constituents would be of the highest value.

The necessary apparatus could be obtained for £20 or £30, and no student who devotes sufficient time to the subject need fear to be disappointed in obtaining results which will certainly be interesting and possibly important.

¹ The following compounds have not been attacked: HF, H₂Se, H₂Te, AsH₃, SbH₃, C₂H₂₂₊₂, SiH₄, SiF₄, SiCl₄, SiBr₄, BH₃, BF₃, BCl₃, BBr₃, B(C₂H₅)₃, CF₄, CHF₃, CH₂F₂, CH₂Cl₂, NO₂, (N₂O₃), HNO₃, NaNH₂, KNH₂, NH₃O, PF₃, PF₅, PCl₃, PBr₃, PI₃, P₂O₅, POCl₃, PSCl₃, H₂SO₄. The following ought certainly to be redetermined: SeCl₄, VOCl₃, SbCl₃, SnCl₄, TiCl₄, HCl, HBr, HI, H₂O, H₂S, C₂H₁₂, CH₃Cl, CHCl₃, PCl₅, CS₂.

RÖNTGEN RAYS

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IN 1895 Prof. W. C. Röntgen of Warburg discovered, it may be said almost accidentally, the rays which now bear his name. During some experiments on the passage of an electric discharge through an exhausted tube, in which the gas pressure was so low that the glass walls glowed with vivid phosphorescence, he noticed that a screen of barium platino-cyanide in the neighbourhood of the tube shone out brightly. By the interposition of obstacles he was at once able to attribute the sharp shadows produced on the screen to the interception of some type of "ray" which proceeded in straight lines from the walls of the tube. Further investigation revealed the fact that these X rays—as Röntgen called them—invariably attend the stoppage of cathode rays by solid bodies.

But the distinctive feature of the Röntgen rays—as they are now generally known—is (as Röntgen himself noticed) their ability to penetrate in different degrees many substances quite opaque to ordinary light. The extent to which suffering humanity has benefited by the application of this property in therapeutics and medical problems can scarcely be estimated. As a measure of the penetrating effect, Röntgen in one of his Memoirs propounded his "density law"—that the absorbing powers of different materials are roughly proportional to their thickness and density.

In addition to this property of exciting fluorescence in suitable materials, Röntgen ascertained that the X rays affect a photographic plate in the same manner as light. J. J. Thomson and others found in 1896 that they ionise or impart temporary electrical conductivity to a gas which, in the absence of the rays, is a nearly perfect insulator. While most of the earlier work on the Röntgen rays depended on their photographic or fluoroscopic actions, practically all recent investigators have

utilised their ionising properties for purposes of measurement, though some methods have also been devised which take measure of the heating produced when the rays are absorbed by a metal.

The nature of the rays.—There are very few scientific discoveries which have excited the imagination and interest of the world in the way that Prof. Röntgen's announcement did. Speculation was rife, and many were the theories put forward of the nature of the new rays.

By some they were identified with extremely short light-waves, but these were met with the argument that nothing corresponding to regular optical refraction or reflection has ever been detected with X rays. It is questionable, too, as will be seen on further reading, whether any genuine diffraction effects have been observed.

On the other hand, in many of their properties the rays strongly resemble the cathode rays; but no observer has ever noticed the slightest deviation in a magnetic or electric field, a result which indicates the absence of a free electric charge. In this particular, then, there is an outstanding difference between the X ray and its creative agency, the negatively charged cathode ray.

Röntgen himself proposed in the third of his Memoirs a theory of neutral material particles. It did not receive general acceptance, for it was then felt that the difficulty of accounting on such a theory for the great penetrating power of the rays was insuperable.

Two theories at present hold the field: the ether pulse theory put forward by Stokes in 1897, and since elaborated by J. J. Thomson and Sommerfeld; the neutral pair theory which has lately received the advocacy of Prof. Bragg. On the one view the Röntgen rays are pulses of electromagnetic disturbance propagated with the velocity of light through the ether, on the other they are doublets made up of a positive and an equal negative charge travelling with great velocity, and possessing energy and momentum. We shall for the present withhold the discussion of these theories, and proceed on lines directed to some portions of the experimental side of the subject which have intrinsic or theoretical interest.

A Röntgen-ray tube.—The modern form of X-ray tube has a concave cathode, with the object of focussing the cathode

rays on the target or anticathode. This latter is placed at 45° to the beam of cathode rays, and is made of some metal of high atomic weight, usually platinum or tantalum: as it tends with a heavy discharge to get very hot, it is therefore often water-cooled. An aluminium anode is joined outside the tube to the anticathode; in the event of the discharge momentarily reversing, the aluminium electrode rather than the platinum takes on the function of temporary cathode, and there is, in consequence, less sputtering of the metal and but slight blackening of the walls of the tube.

As is well known, the pressure in a Röntgen-ray tube tends to become lower with continued running of the discharge. This "hardening" is attributed to absorption by the electrodes, and, as Campbell Swinton has shown (1907), to the driving of the gas into the walls. The effect is nowadays usually met by sealing into the bulb a small platinum or palladium tube, closed at its outer end; by the application of a flame to the end of this tube a small quantity of hydrogen diffuses through the hot metal, and the pressure in the bulb can be restored to a convenient value. Another device for securing constancy of pressure would be to join to the bulb a tube containing coconut charcoal, which can be immersed in liquid air. By saturating the charcoal with gas at the pressure desired, the vacuum can be maintained unaltered for any length of time, no matter how intense the discharge.

Tantalum anticathodes do not appear to harden the tubes so much as platinum; they have too the advantage of a far higher melting point (Pt 1750° C., Ta 2910° C.). The radiation from tantalum contains rather more soft rays, and about ten per cent. less hard rays than the radiation from platinum under the same conditions.

The rays from an X-ray tube are not uniformly distributed about the anticathode, as was formerly supposed. W. R. Ham, at Chicago (1908), and H. Bordier, in Italy (1908), have recently shown that there are large variations in the distribution. In a plane determined by the beam of cathode rays and the normal to the anticathode, the intensity is a maximum in a direction at about 60° from the normal, and falls off roughly by a cosine law from this direction: in a plane at right angles to this, the intensity diminishes much more slowly.

It may be said that the intensity or quantity of the rays given

out by an X-ray tube depends approximately on the current through the tube, while the hardness or penetrating power is a function of the potential difference applied.¹ The character and output of the radiation thus depend greatly on the pressure at which the discharge passes: a low pressure means a large electric intensity in the tube, fast cathode rays and hard Röntgen rays.

Emission of Röntgen rays.—Röntgen, in 1896, found that the rays from an anticathode of platinum are more intense than from one of aluminium in the same tube. The writer obtained (1907-8) a relation between the atomic weight of a metal used as anticathode and the intensity or quantity (measured by an ionisation method) of the hard rays it emits—rays which have passed through about 2 mm. of aluminium. The result is graphically shown in fig. 1, and, as will there be seen, the intensity of radiation and atomic weight increase together: the two are indeed roughly proportional. It is interesting to note that the rays from all the metals are, at this stage, of the same quality—very little change in the relative intensities is produced by increasing the thickness of the aluminium screen. Thick screens of other metals yield much the same sort of curve, modified slightly in the neighbourhood of the atomic weight of the radiator. As the potential on the tube rises, the heavy atoms increase their intensity values somewhat relative to those of the lighter atoms.

The rays from the different radiators were also measured when they were cut down only by a very thin aluminium window fixed in the Röntgen-ray tube. The order of the intensities—which is not that of the atomic weights—agrees with that derived by Starke for the relative numbers of cathode rays returned from the surfaces of different metallic reflectors.

Adams' experiment.—As Birkeland was the first to show, in 1897, the different cathode particles in a discharge produced by an induction coil have a wide range of velocities, and under the influence of a magnet yield, owing to the varying deviations, a visible "magnetic spectrum" on any phosphorescing surface. J. M. Adams, at Harvard University (May 1907), has made a neat application of this, to show that a hard X ray is associated

¹ The matter is however more complicated than this. See, for example, Duddell (*Röntgen Society's Journal*, July 1908).

with a fast cathode ray. The cathode rays from the concave cathode C (fig. 2) were, by the application of a magnetic field (at right angles to the plane of the paper), spread out into a band along the anticathode A B. A small hole S in a lead screen allowed some of the Röntgen rays produced to pass through, and thus on the principle of the pin-hole camera a

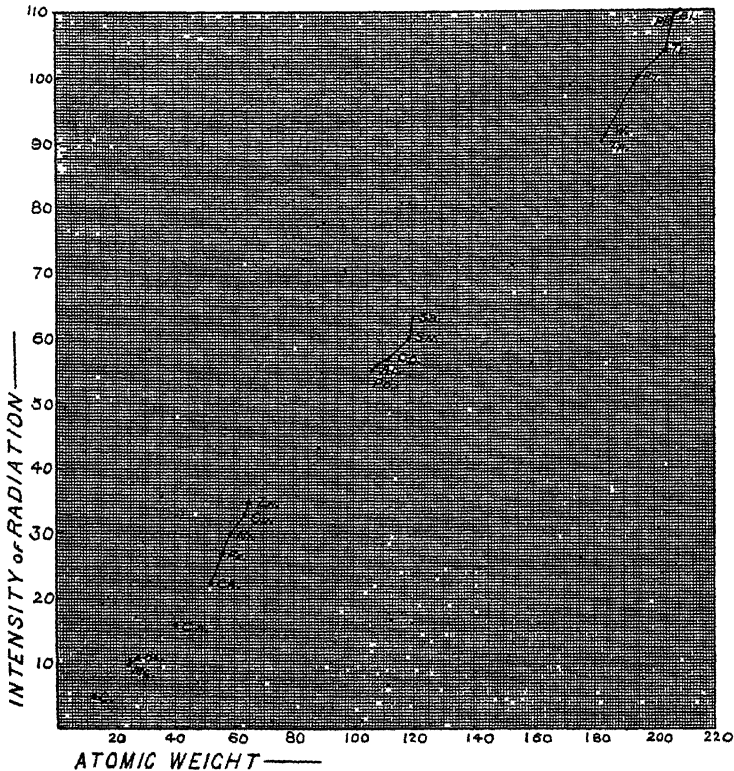


FIG. 1.

spectrum of X rays was obtained on the remote side of the screen. The rays along SD corresponded to the fastest-moving particles which were incident at A, while along SE the rays were those generated by the impact of the slowest-moving particles at B. Narrow slits placed in the pencil of rays allowed different parts to be examined by the radiomicrometer; it was found by the interposition of metallic screens that all

the rays emerging through any one slit had the same quality, and that those produced by the swiftest cathode particles had the greatest penetrating power.

Röntgen rays heterogeneous.—About 1897 it was noticed by a number of observers that the Röntgen rays from a coil-driven tube do not satisfy a test for homogeneity, in that their quality, judged by their penetrating power for various substances, is

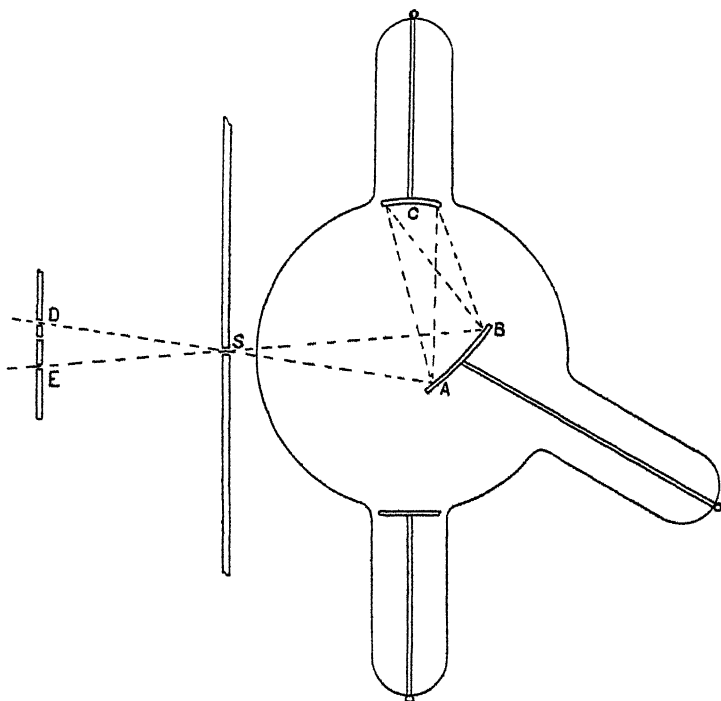


FIG. 2.

changed by transmission through metallic sheets. That a range of hardnesses of X rays should accompany heterogeneous cathode rays would of course be expected. The rate of cutting down of the Röntgen rays is more pronounced when the interposed screen is thin than when it is thick (the soft rays being sifted out first), or, as it will be convenient to regard it, the coefficient of absorption (λ) diminishes as the thickness of screen increases and harder rays are dealt with. λ measures the fraction of the intensity absorbed by unit thickness of screen,

and this definition leads us to the exponential relation for a homogeneous beam

$$I = I_0 e^{-\lambda d}$$

in which I_0 is the intensity of the incident beam and I that of the beam emerging from the other side of a screen of thickness d . It will be noticed that $1/\lambda$ is the distance to which the rays penetrate in the substance before their intensity is reduced to $1/e$ of its original value ($e = 2.72$). The larger the value of λ the more marked is the absorption of the rays.

Absorption and Transmission of Röntgen rays.—As mentioned above, Röntgen enunciated, as a convenient rough working rule, that the absorbing powers of different materials are proportional to their thickness and density. If this were true, then the absorption for any given mass of unit cross-section would be independent of the material: in other words, $\frac{\lambda}{\rho}$ would be a constant for all substances, ρ being the density and λ the absorption coefficient.

Benoist, in 1901, measured the absorption of a definite beam of Röntgen rays, by a great number of elements whose thicknesses were contrived so that each produced the same amount of absorption. For our purpose it will be convenient to express his results in terms of what has now become the standard notation. Values proportional to a mean $\frac{\lambda}{\rho}$ for the material, over the region of absorption, have been derived from Benoist's results. In fig. 3 these values, obtained with hard Röntgen rays from a platinum anticathode, are plotted against atomic weight of screen. It will be seen that $\frac{\lambda}{\rho}$ increases with the atomic weight,¹ and increases more rapidly in the region of low atomic weights than of high. With soft rays the curve is relatively higher in its earlier stages. Kaye (1908) has shown that the shape of this $\left(\frac{\lambda}{\rho}\right)$ curve, besides depending on the range and degree of absorption, is largely dependent on the material of the anticathode which generates the rays. For example, the curve is much straighter for a radiator of aluminium than for

¹ The statement is also true for the soft γ rays of radium. For hard γ rays, a density law holds and $\frac{\lambda}{\rho}$ is constant.

one of platinum working under the same conditions. When screen and anticathode are of the same material, $\frac{\lambda}{\rho}$, or the absorption per unit mass of unit cross-section is relatively low. This evidence of special transparency is there, but is not so marked when the two have atomic weights which are closely adjoining.

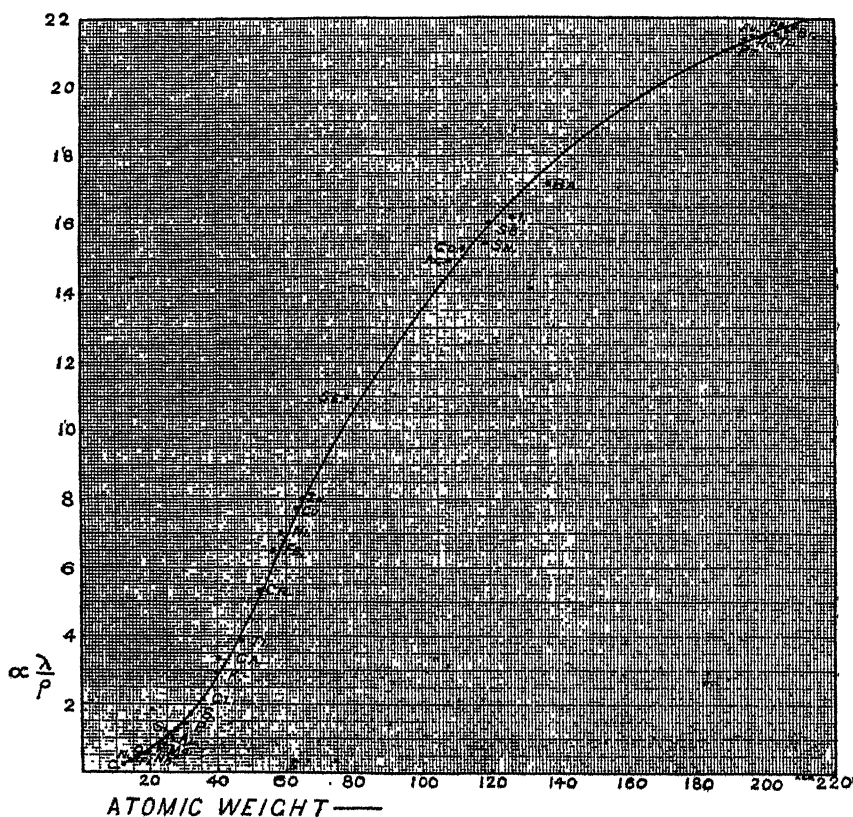


FIG. 3.

Such "selective transmission" is especially pronounced in the case of the elements of the chromium-zinc group, for which the $\frac{\lambda}{\rho}$ curve has to be modified by the addition of a sharp minimum in the neighbourhood of the atomic weight of the radiator.

Selective Absorption and Transmission.—It has long been known that Röntgen rays which have passed through a metal sheet acquire a higher penetrating power for further sheets of

the same or any other metal: the result has usually been ascribed to the selecting or sifting out of certain rays from the original heterogeneous bundle. Walter, in 1905, noticed the effect to be marked in the case of the metals of the silver group.¹ His investigations led him to believe that the rays are actually transformed by the screen in such a manner as to render them more penetrating for any subsequent material. Sagnac claims that his experiments on the reversing of composite screens point to a similar conclusion, though his results are not supported by Adams (1907). The latter, in his experiments mentioned above (p. 302), obtained both photographic and thermal evidence of the special transparency of different metals to certain rays. Haga (1907) has also noticed a similar selective absorption in the case of carbon and ebonite.

It may be pointed out that the statement above as to the increased penetrating power of a beam of rays after transmission is not perfectly general; for Kaye recently showed that when screens and anticathode are of the same metal, an exponential absorption is exhibited over a considerable region. This sign of homogeneity disappears when screen and anticathode are unlike, and is replaced by the type of absorption usual with Röntgen rays, that is, one in which the coefficient of absorption shows a gradual diminution as the screen thickness increases. It is thus unsafe to conclude that such a type of transmission is indicative of heterogeneity: a screen of another metal may yield quite a different conclusion.

For screens of like metal to the radiator the rays show special penetrating power, so that not only is the transmission exponential but selective. The effect becomes less and less marked as the screens increase in thickness, and harder and harder rays are concerned. This is in accordance with what

¹ Prof. Walter used Benoist's "Scale of Hardness" in some of his experiments. This consists of a series of aluminium plates of a variety of thicknesses, surrounding a central silver plate. By a photographic or fluoroscopic method a match is obtained between the silver plate and one of the aluminium plates, when the rays to be tested are sent through the scale: the thickness of this aluminium plate is taken as giving a measure of the hardness of the rays. The instrument has its practical value on account of its rapidity, but the interpretation of its results has led some observers into confusion. In quantitative measurements of hardness it is much better to employ a method which will note the changes of intensity that accompany the use of screens of one metal (preferably one of low atomic weight) alone.

has been found for the γ -rays of radium, which ignore atomic structure, even in the case of the heaviest atoms.

Logarithmic curve of transmission.—When screen and anti-cathode are of the same metal, it is interesting to consider for a moment the curve of transmission in which the logarithm of the intensity is plotted as ordinate against thickness of screen. The main portion of the curve is a straight line. It is steepened in its earlier portion, and is succeeded by a region in which the slope gradually diminishes as the thickness of screen increases. The straight-line portion of the curve displays, of course, an exponential absorption: its slope gives a measure of the absorption coefficient. An explanation of the initial steepening would be furnished if a certain amount of soft radiation were present. This being the case, the preliminary rapid decrease could be eliminated by first inserting a screen of some other metal thick enough to remove all the soft rays. But on trying the experiment it is found that the general shape of the curve is unchanged, the kink still remains. In fact, if one gradually builds up a composite screen of a number of different metals, the logarithmic curve consists of a series of discontinuous steps made up of an initially steep and a subsequently flat portion for each metal.

It is clear that the results are not due to the presence of any soft radiation, but we can find a ready explanation of the early steepness in the curve if we consider the effects of secondary radiation. As is well known, when Röntgen rays encounter a metal surface, secondary radiation in part softer than the primary is generated in varying degree. We should therefore expect that the primary radiation transmitted by any screen will be accompanied by a certain amount of secondary radiation from the screen itself. With thick screens none of the emerging secondary radiation comes from below a certain depth, that proceeding from greater depths is absorbed: thus the transmitted primary radiation is augmented by a proportional amount of secondary radiation. But for screens which are thinner than this layer, the emergent secondary radiation, not having suffered the full absorption, is proportionately larger in amount relative to the primary. Consequently, until the screen attains a certain thickness, the intensity of the transmitted beam will be relatively higher, and the curve of transmission steeper than in the region of thicker screens. It is apropos to remember that a similar

early steepening of the logarithmic curve was obtained by McClelland, when working with the β -rays of radium; for the same metal the amount of the steepening agrees with the writer's value for X rays.

If the incident primary beam be not very hard, the whole of the rays are transmitted exponentially, and the third portion of the curve is undetectable. As the incident primary beam increases in hardness, this third portion, indicative of heterogeneity, increases in extent and amount, at the expense of the intermediate homogeneous region.

When screen and radiator are of different materials the logarithmic curve of transmission has throughout a gradually diminishing slope indicative of complete heterogeneity: no region of exponential absorption can be traced. The result is probably to be attributed to the large amount of scattering (or deviation from their original direction) which the rays undergo when they encounter a screen which presents very different atomic groupings to those of their parent anticathode.

Secondary radiation.—When Röntgen rays come into contact with matter, two classes of secondary rays are generated—one a corpuscular radiation, the other a Röntgen radiation akin in nature to the primary.

1. *Secondary corpuscular radiation.*—Innes (1907) has shown that in the case of metals subjected to a beam of Röntgen rays the emitted negative electrons have a range of velocities in centimetres per second of about 6×10^9 to 7.5×10^9 for soft rays and 6×10^9 to 8×10^9 for hard rays. Their number depends on the intensity of the rays, their velocity does not: the speed does, however, appear to increase a little with the atomic weight of the metal. It is interesting to note that the velocity is of the same order as those of the cathode particles in an X-ray tube.

The distribution of these attendant corpuscles is not uniform. Cooksey (*Nature*, April 2, 1908) found that the quantity of electrons proceeding from the near or incidence side of a metal plate is about 50 to 90 per cent. of the number leaving the "emergence" side. More cathode rays are emitted from both surfaces of metals of high atomic weight than from those of low. Prof. J. J. Thomson (1906) showed that the intensity of the "incidence" secondary cathode radiation increases steadily with the atomic weight of the radiating substance.

2. *Secondary Röntgen radiation.*—These secondary rays pro-

duced by matter in all its states have been the subject of work of a good many observers. Barkla's earlier work led him to the result, since widely quoted, that

(1) With elements of low atomic weight the secondary radiation is wholly of the same type as the primary; that, in fact, the secondary radiation may be regarded as so much scattered primary.

(2) With elements of high atomic weight the secondary rays are not all of the same type as the primary; a portion of them is very much softer.

Barkla has since found (*Nature*, Feb. 13, 1908) that this perfect scattering with light elements is only true when the primary rays are very soft; with very hard primary rays the purely scattered radiation disappears, and is replaced by a softer radiation, with a different distribution. Moreover, many of the elements of high atomic weight will, under suitable conditions, yield a scattered radiation, just as the lighter atoms do. It thus appears probable that, with a suitable intensity of the primary beams, all elements behave similarly. The soft secondary radiation is in most cases characteristic of and specially penetrating to its parent element.

The secondary radiation from gases is of interest on account of the comparative simplicity of the conditions. Barkla (1903-4) concluded that the ratio of the quantity of the secondary radiation to the quantity of the primary is independent of the hardness of the primary, and dependent only on the density of the gas producing it. The result has theoretical importance, but Crowther (1907) has shown that the statement holds only for those elements of low atomic weight (with the exception of hydrogen), and that in general no "density law" can be said to hold. As to the hardness of the secondary radiation, it varies with the nature of the gas; the lighter gases merely scatter the primary radiation, while the heavier gases and vapours transform it into very soft and intense secondary radiation.

Heating effects of Röntgen rays.—The heat produced when Röntgen rays are absorbed by metals was first measured by Dorn (1897). The effects are small, and need a sensitive detector. Rutherford and McClung (1902) used a bolometer, Adams (1907) a radiomicrometer. Angerer (1906) has done noteworthy and comprehensive work on the subject. Bumstead, using a radiometer, obtained results in 1906 which seemed to

show that more heat was generated when the rays were totally absorbed by lead than by zinc; it was suggested that the results were due to the liberation of atomic energy, and thus lent support to the explosive theory (p. 316). However, Bumstead's later work (1908) and that of Angerer (1907) have not confirmed the results, and the assumption that the heat developed is proportional to the energy in the rays absorbed may be regarded as legitimate.

The available energy seems in all the different reactions—thermal, photographic, ionisation, etc.—to be proportionally larger with soft than with hard rays.

Polarisation of Röntgen rays.—Barkla in 1905 showed that the scattered secondary Röntgen rays from a plate bombarded by primary rays exhibit an asymmetry of distribution in a plane at right angles to the direction of propagation of the primary beam. This asymmetry is such as to make the intensity a minimum in a direction parallel to the cathode rays in the X-ray tube, and a maximum in a direction at right angles to this. For a carbon radiator, for example, these intensities have, with soft primary rays, the relative amounts 1:1·2. The values approach equality, or in other words the extent of the polarisation diminishes as the hardness of the primary ray increases. While we are concerned with the distribution of these “incidence” scattered secondary rays it is worth noting that their intensity along the path of the primary rays is for soft rays about double that in a plane at right angles: with hard rays the ratio diminishes a little.

If a secondary beam is allowed to fall on a reflector, tertiary rays are generated: their distribution indicates an almost complete polarisation of the secondary rays. Barkla measured his intensities by an ionisation method. Haga (1907) has repeated the experiments using a photographic method. He has obtained striking confirmation of Barkla's results for the polarisation of secondary beams, but his experiments do not indicate any polarisation in the case of the primary rays.

This polarisation or one-sidedness of a Röntgen ray has theoretical importance, and will be brought forward for discussion when the rival theories are considered.

Velocity of Röntgen rays.—In 1906 Marx published the results of an ingenious and difficult investigation on the speed of the Röntgen rays. He excited the rays by means of waves from an

electrical wire system, and these waves also charged to a varying potential an insulated plate on which the rays fell. The consequent emission of secondary corpuscles from this plate was measured by a Faraday cylinder connected to an electrometer; the amount was obviously controlled by the phase-relation between the potential of the plate and that of the cathode of the Röntgen ray bulb. If the various distances and wire lengths were adjusted so that the charge received by the Faraday cylinder was (say) a maximum, then it was found that if the distance of the X-ray bulb from the insulated plate was altered by a certain amount, the wire along which the waves travelled to the plate had to be altered by the same amount to restore the maximum. Thus, according to Marx, the Röntgen rays travel with the same velocity as electric waves along wires, and therefore with the velocity of light.

Franck and Pohl (1908) have repeated Marx's experiments, and question the validity of his method. Their criticisms, which are supported by a large number of experiments, throw very serious doubts on Marx's results. Their objections are:

(i) That the potential change of the insulated plate was insufficient to alter the emission of corpuscles by more than ten per cent.

(ii) That ions were produced directly by the Röntgen rays in the gas round the insulated plate; these were comparable in amount with the electrons from the plate, and, being measured with them in greater or less degree, confused the sharpness of the readings. They found that the charge received by the cylinder might indeed, by altering the gas pressure, be changed in sign.

(iii) That Marx's results were really due to the Himstedt effect. Himstedt has shown that as one approaches a discharging point subjected to an alternating potential one passes through zones in which the charge in the air is first negative, then neutral, and finally positive. These zones have positions depending on the potential of the point and the gas pressure. Marx's readings, so Franck and Pohl maintain, only indicated that the amplitude of the waves was such as to bring the Faraday cylinder into the neutral zone of the oscillations to which the insulated plate was subjected. By altering the conditions, Franck and Pohl were able to obtain widely varying values for the velocity of the Röntgen rays.

It is impossible, in the space at disposal, to give more than a notion of the points involved; the reader is referred to the original papers. Marx has replied to his critics; but one is left with the unsatisfactory feeling that much reliance cannot be placed on the value that experiment has so far given us for the speed of the X rays. If such velocity were certainly known, it would be of great value in discriminating between the two theories of the Röntgen rays.

Diffraction of Röntgen rays.—A good deal of ingenuity has been exercised in testing whether, as in the case of light, there are, on the boundaries of the shadows cast by small obstacles, variations in the intensity of the X rays, corresponding to the well-known diffraction fringes. Röntgen (1898) could not satisfy himself on the point. Haga and Wind (1899-1901) worked with a V-shaped slit, a few thousandths of a millimetre broad at its widest point, and obtained in their photographs of the slit broadenings of the narrow part of the image: if the effect were one due to diffraction, the same amount of broadening with light would be associated with a wave length of about 1.3×10^{-8} cm. On the pulse theory of the Röntgen rays, this would be the order of the pulse thickness of the particular rays used.

Walter and Pohl (1908) have recently repeated Haga and Wind's experiments. The width of the slit-image, they find, depends on the amount of energy sent through the slit, so that different times of exposure give rise to different images. Haga and Wind's measurements they consider to be in error, owing to contrast effects produced by prolonged photographic development and the method of measuring the slit-image on the plate. Walter and Pohl conclude that the diffraction of the X rays has not been established, and that the pulse thickness—if such exists—is well under 10^{-8} cm.

We are now in a position to consider in turn the two theories which offer interpretation of the nature and behaviour of the Röntgen rays.

The Ether Pulse Theory.—Sir George Stokes, in the Wilde Lecture given before the Manchester Philosophical Society on July 2, 1907, propounded the pulse theory of the Röntgen rays. Of the nature of his conception we read that “. . . when these charged molecules (from the cathode) strike the target, it is exceedingly probable that by virtue of their charge they produce some sort of disturbance in the ether. This disturbance . . . or

‘pulse,’ as I will call it, in order to distinguish it from a periodic undulation, would spread in all directions, so that . . . according to the view here put forward, the Röntgen emanation consists of a vast and irregular succession of isolated and independent pulses, starting respectively from the points and at the times at which the individual charged molecules projected from the cathode impinge on the target.” J. J. Thomson was then about to give to the world the results of his brilliant researches on the mass and charge of the cathode rays, and we now know that for “molecules” we must substitute the much smaller electrons or corpuscles. Thus, very much on the analogy of the generation of a sharp crack or pulse of sound by the sudden stoppage of a whip-lash, Stokes conceived the origin of a Röntgen pulse. Furthermore, “. . . we know of no reason beforehand forbidding us to attribute an excessive thinness to the pulses which the charged molecules excite in the ether”; and to this pulse width, small compared even with that of the wave length of light, Stokes attributed some of the differences between ordinary light and Röntgen rays.

J. J. Thomson has elaborated Stokes’ original theory both analytically and by the method of Faraday tubes. Briefly, the developed theory postulates that a Röntgen ray is a thin pulse of intense electric and magnetic force, which travels outwards with the speed of light in all directions from the place where the motion of a charged particle is arrested or changed. Its thickness depends on the abruptness of arrest: more energy is associated with thin pulses than thick ones, not only because the thinner pulses are produced by the stoppage of more rapidly moving cathode particles, but also because a larger proportion of the kinetic energy of fast-moving particles is converted into radiant energy owing to the shorter time of impact. (We here assume that the distance in which the particle is brought to rest is approximately the same whatever its velocity.) The electric intensity in the pulse is, of course, ever weakening as the pulse widens.

Since in a Röntgen tube the cathode rays are all travelling in the same direction, it is to be expected that the electric forces in the resulting pulses will lie in planes passing through that direction, and not in planes perpendicular to it—that is to say, the Röntgen rays should be polarised in a plane at right angles to the direction of motion of the cathode rays. This one-sidedness fits in with Barkla’s result (p. 311) that, in a plane perpendicular

to the path of the Röntgen rays, the scattered secondary rays preponderate in a direction at right angles to the original stream of cathode rays. That the rays are found not to be completely polarised can be ascribed to the fact, supported by other evidence, that the cathode rays are not stopped in a single collision, but describe random and irregular paths along many directions before coming to rest.

In many respects, besides that of speed, the pulses resemble waves of visible light; both consist of electric and magnetic forces at right angles to each other and to the direction of propagation, and both can exert a pressure on the medium in the direction of propagation. This pressure in the case of light is the pressure of radiation which has been measured by Lebedew, Nicols and Hull and Poynting: in the case of the X and γ rays the effect may be traced on the distribution of the secondary electrons produced when matter is encountered. The ionisation produced by the rays represents the effect of the electric force in the wave front, in causing the ejection of electrons out of encountered atoms.

The ionising effect of ultra-violet light on gases has been demonstrated by Lenard (1900), and more recently by J. J. Thomson (1907): the much more marked effect due to Röntgen and γ rays is, of course, well known. We are thus introduced to a resemblance between the rays and light in which the effects differ only in degree.

Now, even in the most favourable circumstances, only an exceedingly small fraction—less than one in a billion—of the molecules of a substance subjected to the Röntgen rays becomes ionised. If all the molecules are equally exposed to the rays, as would be implied by the above form of the pulse theory with its continuous wave front, those which are ionised must be in a state very far removed from the average. The abnormal condition of such molecules cannot be attributed to the possession of an exceptional amount of kinetic energy, for the kinetic theory of gases would then require that the ionisation in a gas (if its molecules are subject to the Maxwell-Boltzman law of distribution) should vary rapidly with the temperature: this is entirely opposed to present experiment, which indicates that the ionisation is independent of the temperature.

The most plausible of the other assumptions that are open to us, if we are to retain the continuous pulse, is that the ionisation

of an atom is determined by its internal conditions. We have reason for believing from the phenomena of spontaneous radioactivity that atoms possess large stores of internal energy, in amounts which are not readily susceptible to extra-atomic conditions (such as temperature), but which probably vary a good deal from atom to atom. If the possession of an abnormal amount of internal energy meant diminished stability then it might easily happen that only abnormal atoms would be ionised: as these form only a very small fraction of the whole, the fewness of the ionised atoms would be accounted for. Thus far, well and good. But, if we now take up the consideration of the experimental evidence which secondary radiation provides, we find it useful, if not necessary, to be able to invoke still further the aid of this atomic energy.

When ultra-violet light Röntgen and γ rays fall on metal surfaces they cause the emission of corpuscles. The effects strongly resemble each other, as will appear; for comparison's sake the results are summarised.

(1) With ultra-violet light the emitted corpuscles from metals have a mean velocity of about 5×10^7 cms. per second. The intensity of the light controls the number of the particles, but not their velocity, which is, however, according to Ladenburg (1907), directly proportional to the frequency of the ultra-violet light.

(2) Röntgen rays cause electrons to be emitted from metals with velocities which average about 7×10^9 cms. per second. The intensity of the rays affects the number of the corpuscles; their velocity is independent of the intensity of the rays, but increases somewhat with their hardness, and possibly a little with the atomic weight of the radiator. The corpuscles preponderate in the direction of propagation of the Röntgen rays. It should be noticed, in passing, that the speed is much the same as those of the cathode rays in an X-ray tube.

(3) In the case of the γ rays (from radium) the expelled β particles from metals have velocities approaching those of the primary β rays from radium, which have an average speed of about 2.6×10^{10} cms. per second. The secondary β particles have a maximum intensity in the direction in which the primary γ rays are moving. Their speed is independent of the nature of the metal.

The Explosion Theory.—Of these results, that which remarks on the independence of the velocity of the corpuscles on the

intensity or quantity of the rays calls for first consideration. This result, standing alone, has often been interpreted as indicating that the corpuscles from the metal are not expelled by the electric forces in the wave front, but by a sort of radioactive explosion of some of the molecules which have been put in an unstable state by the incidence of the light, which on this view merely acts as a trigger to start the explosion: thus both the energy and the material of the corpuscular radiation are furnished by the atom. We know that radioactive substances emit high-speed electrons, and indeed there is other evidence to support this theory of atomic disintegration. J. J. Thomson (1908) measured the maximum velocity of the secondary cathode rays which are produced when primary cathode rays move through gases; he found it to be independent of the gas and of the speed of the primary rays, although the latter was widely varied. Füchtbauer (1906) got almost the same value for the velocity of the secondary cathode rays from a number of different metals subjected to primary cathode rays or canal-strahlen.

But, coming back to the case in point, we have now to explain why, as set forth above, the speed of the electrons is not independent of the quality of the primary rays. Why should the speed be great when the light-waves are short and the pulses thin, if their only effect is that of a trigger action? And further, why should the electrons pursue, in the main, the original direction of propagation of the pulses? Nor do the difficulties end here. If the electrons owe their velocity to atomic disintegration, we should expect their speed to vary from atom to atom much more than experiment indicates, and as it does in the case of the radioactive substances. Thus the explosion theory cannot be said to score many successes, and, on summing up, it must be conceded that the energy of the secondary electron does not come, at any rate entirely, from the atom.

We are left to suppose that the energy is imparted by the Röntgen or light rays, since it varies in a continuous manner with their quality. Now it can easily be shown that the energy of a pulse, if spread over an ever-widening space, is utterly insufficient to provide the energy required for the secondary electron, and thus the pulse theory as we have been regarding it requires modification.

The discontinuous wave front and pulse theory.—We are led to the development which not only the theory of pulses but the

electromagnetic theory of light has received at the hands of J. J. Thomson. On the electric theory of light as usually given, it is tacitly assumed that the electric force is everywhere uniform over the wave front, that there are no inactive spaces, and that the front has no structure. Prof. Thomson suggests that the energy in both Röntgen and light rays is localised in limited regions, that the fronts of the disturbance are porous and possess a structure which may be appreciated by the analogy of a number of bright specks on a dark ground.

The existence of a structure in the wave front implies a structure in the ether, which it is assumed has disseminated through it discrete lines of electric force in a state of tension; light travels along these lines as transverse vibrations, Röntgen rays as pulses. Thus energy travelling outwards with the wave is not distributed uniformly over the wave front, but is concentrated in those parts which are, for the time being, disturbed by pulses travelling in straight lines, with the speed of light, along some or other of the lines of force. "The energy is, as it were, done up into bundles or units, and the energy in any particular bundle does not change as it travels along the line of force." The rays diminish in intensity with increasing distance owing to the wider separation of the bundles and not to the enfeeblement of individual units. The distribution of energy is thus very much like that contemplated on the old emission theory of light, according to which the energy was located on moving particles sparsely disseminated throughout space.

Such a discontinuous wave front provides a ready explanation of the small proportion of molecules ionised both by ultra-violet light and Röntgen rays. If a unit by impinging on a molecule makes it liberate a corpuscle, it will hand over to the corpuscle its whole store of energy and the freed electron will start with the same velocity, no matter where the encounter occurs; thus the velocity of the electrons would be independent of the intensity of the light. And further, if the energy in the bundle increases as the wave length or pulse thickness diminishes, the secondary electron liberated by a short wave or thin pulse would be expected to have a high velocity. Since the thickness of a pulse (presumably less than 10^{-8} cm.) is very small compared with the wave length of even ultra-violet light (say about 10^{-6} cm.) we should for the same reason expect much greater energy in the units in the γ or Röntgen rays than in the case of ultra-

violet light. This is indicated by the respective velocities of the secondary corpuscles, which with γ rays have about two million times, and with X rays about twenty thousand times the energy with ultra-violet light.

The greater the frequency of the light the greater is the energy in each unit, and it may thus happen that a molecule may set a superior limit to the range of wave lengths of light which can ionise it. This would explain the superior ionising ability of ultra-violet light over visible light.

As the units or bundles possess momentum as well as energy, there is no difficulty in accounting for the fact that, when matter is encountered, the secondary electrons preponderate in the direction of propagation of the rays.

It is to be noticed that, on this theory, light will have an extremely coarse structure; for Röntgen and γ rays the coarseness will be even more marked.

The Neutral Pair Theory.—Prof. W. H. Bragg in May 1907 suggested for Röntgen and γ rays a constitution obtained by the close association of a positive and a negative particle which rotate round each other. The negative particle consists of a cathode or β ray; the positive was at first regarded as of atomic dimensions, but Prof. Bragg's later work has led him to give preference to the, as yet unisolated, positive electron.

Such a pair would display at once some of the features of a Röntgen ray, such as the absence of deviation in an electric or magnetic field, and of refraction. The large penetrating power of the rays is explained partly by the high velocity, and partly by the weak external electric field of the pair. The closer the two components are together, or in other words the smaller the moment of the doublet, the more limited will be the field, and the greater the penetrating power. The latter, of course, also increases with the speed. The ionising properties depend on the strength of the external field; they will be small with pairs of little moment. Those doublets in which the attachment of the positive to the negative has become loosened, constitute a softer and more ionising radiation.

As to the method of ionising, we have to suppose that the united positive and negative particles passing into an atom are separated if the internal field of the atom is strong enough; the negative flies on unaffected in speed by the field, and the positive is left behind in the atom. This is the explanation of the excess

of secondary cathode rays in the direction of propagation of the X or γ rays.

That the velocity of the secondary corpuscles should increase with harder rays follows at once ; for the faster the doublet, the greater the initial speed of the negative component.

The discrepancy between the number of molecules ionised and the whole number present is explained by the fact that the neutral pair is local in its action, and affects only those molecules which it encounters. Its speed is maintained with little loss during its flight : and we thus arrive at the independence of the velocity of the secondary corpuscle on the ray intensity.

The production, sometimes of true secondary rays and sometimes of scattered rays, when X rays encounter matter, occurs when the pairs striking the atoms are respectively shattered or merely ejected unchanged. But it has also to be noticed that when X rays are incident on a plate the cathode rays leaving the near side of the plate are derived in part from the stream of such particles which was travelling with the X rays before incidence, and which had been formed from the molecular encounters en route. This portion is scattered to an extent which depends on the atomic weight of the plate. The other part, which originates in the plate itself, also varies in amount with the atomic weight. Now the intensity of the "incidence" secondary cathode radiation in the case of X rays, β rays, and γ rays has been shown by J. J. Thomson, McClelland and Kleeman respectively, to follow the order of the atomic weights. The neutral pair theory throws in each case the responsibility for the resemblance largely on the presence of negative electrons in the incident beam.

Prof. Bragg has developed the theory to furnish an explanation of polarisation. If in a Röntgen tube the effect of a cathode particle on the motion of an atom in the anticathode is such that a pair is thrown off, the pair will rotate in the plane which contains the directions of propagation of both the incident cathode ray and the ejected pair. If such a pair falls upon a reflecting surface it will be liable to be taken up only by an atom revolving in the same plane, and if ejected again will continue to move and rotate in that plane. Thus the effects will be very much like those which experiment has disclosed.

The suggestiveness of the fact that the secondary cathode rays due to X rays have velocities of the same order as those of

the primary cathode rays in the Röntgen tube has been pointed out by Bragg. He inclines to the view that when the primary cathode particle strikes the anticathode it picks up a positive there, becomes neutral and constitutes an X ray: subsequently it is stripped of the positive and ends up as a secondary cathode ray; thus the identity of the negative remains the same throughout and its speed invariable or nearly so. The fact that a hard X ray is generated by a fast cathode ray is of course in keeping with all this. A similar argument holds in the case of the γ rays.

The close relationship between the cathode and Röntgen rays would provide an explanation of the mechanism of absorption; for a Röntgen ray which had been deprived of its positive and become a secondary cathode ray would be lost to measurement as an X ray and be absorbed or scattered in the manner of cathode rays.

Experiment has not yet succeeded in isolating the positive electron. Bragg's theory does not, however, postulate its free existence, but only that it can be torn from its attachment in some atom and carried away by a passing corpuscle, and again that it can be left behind in some atom which the pair subsequently traverses.

Röntgen and γ rays.—Before summing up the position, a word as to Röntgen and γ rays. So many points of resemblance exist between the two that their identity of nature can scarcely be doubted. But it is remarkable that, while the impact of cathode rays gives rise to X rays, experiment has not yet been able to show that the impact of β rays occasions the generation of γ rays (Stark, March 1908; Eve, August 1908). Evidence as to the existence of polarisation in primary or secondary γ rays would be most valuable in establishing their kinship to Röntgen rays. As matters stand, probably the most noteworthy difference between X and γ rays, other than that of degree, is that the former arise where the cathode rays are arrested, and the latter when the β rays originate.

The present position.—The "discontinuous pulse" and "neutral pair" theories attribute structures to the Röntgen rays which are so similar in behaviour that present experiment cannot be said to discriminate between them. Both theories account for the high penetrating power, the absence of deviation in electric and magnetic fields, and the lack of any refraction. The pre-

ponderance of secondary electrons in the direction of propagation of the rays would be expected on either view. Polarisation is a natural consequence of the pulse theory, but Prof. Bragg has shown that it is not impossible to account for it on the neutral pair theory. Prof. Thomson's units or bundles of energy possess momentum, so that they have all the properties of material particles, except they cannot move at any other speed than that of light. Indication of diffraction would be looked for by the supporters of the pulse theory. Thus reliable experimental evidence (at present wanting) of velocity and diffraction would be especially valuable.

The resemblance between the electrical behaviour of ultra-violet light and that of Röntgen rays is insisted upon by Prof. Thomson in advocating the discontinuous pulse and wave front theory; but Prof. Bragg finds it hard to believe that a diminution of the width of the pulse, no matter how extreme, can increase the energy of the secondary electron by anything from twenty thousand to about two million times. He regards the action of Röntgen and γ rays as something entirely different from that of ultra-violet light.

As regards the radioactive or explosion theory, we have seen that it is unable to satisfy all the results of experiment. We may, however, note that Wien (1907), from whom this theory has received active support, has, by the application of a theory of Planck's, derived a result which states that the velocity of the ejected secondary electron varies inversely as the square root of the thickness of the incident pulse. This removes one of the failures of the theory—that the speed of the secondary cathode ray depends on the hardness of the X ray—but the other difficulties remain as great as ever.

It is not easy to establish argument for or against either "pulse" or "pair" theory from the standpoint of the relative energies of the primary cathode ray, the Röntgen ray, and the secondary cathode ray; for if in any transformation the energies do not balance, the difficulty may be avoided by invoking the aid of the internal energy of the atom concerned.

Having regard to the heat developed at an anticathode, it does not seem credible that the whole of the energy of a cathode ray can be handed over either to a pair or a bundle; that this latter, encountering some molecule, can, independently of the heat liberated there also, send out a secondary ray with almost

the velocity which the original cathode ray owed to the electric intensity in the discharge tube. The heat energy, of course, may be derived wholly from those encounters in which the ray is merely absorbed, and in which the conditions do not favour a transformation of the ray, with a transfer of energy without any degradation.

As both primary and secondary cathode rays are heterogeneous, it may be, however, that a secondary ray may owe its velocity to an X ray generated by a slightly faster primary cathode ray, the small difference in the energies being that lost in the two transfers. Thus the mean speed of the secondary beam of rays would be a little lower than that of the primary, but the difference might not be marked.

In the discontinuous pulse theory, one has to assume that the whole of the energy of the primary cathode ray goes into a single energy bundle, which in its turn liberates one, and only one, electron, to which it gives up all its energy. Otherwise we cannot, without calling in the aid of atomic energy, provide for the almost equal velocities of the primary and secondary cathode rays.

It may be noticed that the pulse theory demands that the thickness of the pulse or the size of the bundle must not be greater than the diameter of a corpuscle; if it is thicker, only a portion of the energy of the cathode particle is converted into radiant or pulse energy. Now other considerations indicate a value for a diameter of the corpuscle of about 10^{-13} cm. Apropos of this we may record the calculations of Wien (1907) and Stark (1908), who, working on extensions of Planck's theory of radiation, and supposing that no energy is degraded in a primary or secondary transformation, derive values of 6.8×10^{-9} and 6×10^{-9} cm. respectively for the thickness of a pulse.

An ether pulse must accompany an alteration in the motion of a charged particle, if the change is sufficiently sudden. Wien (1905) and van der Waals, jun. (1907), calculate that if the whole of the energy of a cathode ray is to be transformed into pulse energy, it must be completely stopped in a space of about 10^{-10} cm. Thus these different calculations are based on assumptions and data which yield values of more nearly atomic than corpuscular dimensions.

As regards the neutral pair theory, it is difficult to understand the mechanism by which a cathode ray entering an anticathode

can there pick up a positive electron, and without the assistance of atomic energy emerge with its direction reversed, or nearly so, and with a velocity much the same as when it entered. Neither is it easy to imagine an electric field which, prevailing in an atom, can dissolve a doublet, retain the positive and yet not affect the velocity of the negative, which goes on with the speed of the doublet. The part played by the positive component seems a little obscure.

It must be admitted, however, that Prof. Bragg's theory of the close resemblance between the β and the γ rays—the difference being merely one of charge—has several experimental results in its favour. For instance, both β and γ rays produce secondary cathode rays with nearly equal velocities, and it does not seem reasonable that, while in the case of the β rays the secondary radiation consists largely of scattered primary, it should, in the case of the γ rays, be atomic in its immediate origin, as the pulse theory insists.

In reviewing the subject, the mind recalls two analogues to the present position in the history of physics. First when experiment was needed to discriminate between the undulatory and the Newtonian emission theory of light. And again when, in more recent years, the English and Continental schools were at issue as to the nature of the cathode rays.

Now, as then, the choice lies between a material particle and an immaterial disturbance, and once more it is for experiment to decide.

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PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES

PART I

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I

THE changes which have affected the face of the earth since the dawn of recorded history are comparatively few and unimportant. In some regions, as in the British Isles, great tracts of forest and marsh have been replaced by cultivated land, and some few species of wild animals, such as wolves and bears, have been exterminated; but, so far as we can judge, the climate has remained the same, and no movements have permanently disturbed the level of the sea. The recent period seems to have been one of geological repose, affording a peaceful and stable arena for the great drama of human existence. The historian consequently may pursue his researches untroubled by disturbances of the environment, accepting the world as it now is, as that which, so far as he is concerned, has always been. But directly we extend our inquiries into antecedent periods, and endeavour to recall the story of our species from the unwritten past, we are conscious of a new régime: not constancy, but change seems to dominate the environment. The climate loses its stability; it swings slowly to and fro between extremes of heat and cold, of moisture and dryness, in long oscillations several times repeated. Harmoniously with these, successive assemblages of living forms—southern, temperate, northern—faunas of the forest, the tundra, and the steppe—make their appearance in the temperate European zone, disappear to reappear, and then finally vanish, either altogether or into remote regions of the earth.

Even the land itself ceases to maintain its solid firmness, but subsides over larger or smaller areas beneath the waters of the encroaching sea, or in some places rises to greater altitudes, and even shares in the increasing growth of mountain chains.

No doubt, in a retrospective glance we are liable to a deceptive effect of perspective, and events widely separated in fact appear unduly crowded together by foreshortening. We are not, however, altogether without the means of making an appropriate correction for this illusion. The geological scale of time, though far from exact, is sufficiently so for the purpose, and, judged by this standard, the duration of the latest epoch of terrestrial history, known as the Pleistocene, may be regarded as but little short of half a million of years. It corresponds with the chief period of human development, and includes four complete oscillations of climate, each possibly of the average duration of a hundred thousand years.

Of the many changing elements which contribute to the geology of the Pleistocene epoch, climate is one of the most important, and to this, therefore, in the first place, we will turn our attention. The recent existence of a great Ice age was first divined by Schimper, the poet-naturalist, whose enthusiasm fired the imagination and stimulated the researches of the indefatigable Agassiz.

As a result of his investigations, Agassiz announced his belief that the earth had passed at no distant date through a period of extreme cold, when ice and snow enmantled a large part of its surface. Attempts, persisting even down to the present day, have been made to overturn or belittle this conclusion, but with very imperfect success, and it now stands more assured than ever, as is shown by the increasing testimony of concordant observations.

The evidence on which Agassiz based his views was derived, in the first instance, from a study of the Swiss glaciers and of the effects associated with their existence. The contemporaries of Agassiz—Forbes and Tyndall—and subsequent generations of scientific explorers have pursued their researches in the same region; and this land of lofty peaks, which has furnished inspiration to so many great discoverers in other branches of geology, is thus pre-eminently classic ground for the glacialist. Let us then commence our studies in the Alps, and, as a preliminary to further studies, make ourselves acquainted with phenomena now alien to our land.

When Agassiz began his researches, glaciers were but little known, even to the travelled Englishman; now a crowd of summer visitors makes holiday upon them. It matters little to

which of the many glacier systems we direct our attention; perhaps one of the best known is that which contributes to the astonishing panorama unfolded before us from the Gorner Grat. Dominating the scene is an array of majestic snowy peaks. On the extreme left stands the mighty complex mass of Monte Rosa, then the Bretthorn; in front of us the Matterhorn rises in its superb and isolated grandeur; farther to the right come the Dente Blanche, the Gabelhorn, the Rothhorn; and last, the shapely Weisshorn, sometimes regarded as the most complete realisation of the ideal of mountain beauty.

Below lies a wide valley, filled deep with a mass of slowly flowing ice, fed by many tributaries pouring down from the broad snow-fields which sweep around and between the mountain fastnesses. Two main streams—the Grenz and the Gorner glaciers—unite on almost equal terms, and flow together as the Boden glacier, which comes to an end at the upper margin of the Hinter Wald, above Zermatt, where it melts away into the hurrying Visp.

Suppose now that by some magic wand we could wave away all these streams of ice, and dismantle the mountains of their snowy robes, leaving the rocks exposed and bare. A strange and wonderful landscape would then stand revealed; the valleys, as far up as the ice had filled them, would be modelled in smooth and round and flowing outlines, in striking contrast to the rugged forms of the frost-splintered mountain summits. Angular fragments of rock, some of them very large, the remnants of the lateral moraine, would lie scattered over the valley sides, marking the line where the glacier had lapped against its banks; and a heap of debris, confusedly piled together, would stretch across the valley in a crescentic mound, not completely continuous, and looking like the ruins of a great natural dam. This is the terminal moraine, and marks the end of the vanished glacier. Behind it we might see a basin-like depression, in which the glacier had sunk itself by abrasion; within this, rising from its surface, elongated hummocks, or drumlins, of boulder clay, arranged in divergent radiation, conformably with the stream-lines of the glacier.

When we have gazed on the desolate scene long enough to distinguish its principal features, we will descend from our eyrie and examine them more in detail. The smoothness of rounded outline which we have already remarked is found to be due to

the abrasive action of the glacier, which has ground away all the asperities of its bed; crags and jutting rocks have been worn down into rounded bosses (*roches moutonnées*), the smooth surfaces of which are striated by grooves and scratches all running in the direction of the stream-lines of the once-flowing ice.

The drumlins would be found to consist of a tough clay crowded with stones of all sorts and sizes, but bearing very remarkable features by which they are readily distinguished. Originally angular fragments, they are now subangular, their sharp edges and corners having been ground away and rounded off by the ice; their flattened faces are smoothed and polished, and covered with scratches which run in parallel groups, generally in the direction of the longest axis of the stone, but occasionally crossing this direction. The whole assemblage of scratched stones and clay is known as till or boulder clay.

Such, then, are the signs which would be left behind on the disappearance of the ice.

It requires no magic wand to bring about the transformation we have imagined; an amelioration of climate will suffice. Even at the present time the Boden glacier, like so many other great glaciers in Switzerland, is diminishing in bulk; its surface, instead of bulging up, is sagging in like an empty paunch, since the annual snowfall is insufficient to make good the annual loss due to melting away. A general rise of temperature over Switzerland to the extent of 4° or 5° C. would probably drive the snow-line above the mountain peaks, and all the glaciers would disappear.

Let us now suppose that the climate, instead of ameliorating, grows gradually more severe. The Boden glacier will be more richly replenished by its tributaries; it will bulge upwards and downwards, and descend farther into the valley of the Visp; if the mean annual temperature falls low enough—say, 5° C. below the present—it will extend downwards till it reaches the valley of the Rhône. All the glaciers which lie in valleys tributary to the Rhône will similarly enlarge, as will the glacier of the Rhône itself.

If, bearing this possibility in mind, we walk down the valley of the Visp, we shall discover on every side evidence of an ancient extension of the ice, and on the most stupendous scale. The swollen Visp glacier soon became confluent with that which filled the Sass-thal, and their united volume then entered the glacier of the Rhône. This, which now ends close to the Furca,

had then already attained there a thickness of some 5,000 ft., and overflowed the Grimsal pass. Farther down, where the Visp and Saas glaciers entered, it was even thicker. Filling the valley, it pursued its course past the bend at Martigny, and emerged from the Alps to overwhelm, in a great fan-shaped expansion, all the region now occupied by the lakes of Geneva and Neuchâtel; it rose against the flanks of the Jura to a height which shows it to have possessed even at this distance from its source a thickness of 4,600 ft. But it did not terminate here; it surmounted the Jura, and debouched on the plains of France. There it deposited its terminal moraine, which runs in a much indented, but on the whole crescentic, line from Videnne, through Lyon, past Villefranche, to Villereversure, Arlay, Mesnay, Morteau, till it re-enters Swiss territory, between Maiche and Seignelegier, to become continuous farther on with the similar moraine of the great Rhône glacier.

As might have been expected, this increase in volume was not confined to the glaciers of the Rhône valley. All the glaciers of Switzerland were affected in a corresponding degree; and the whole of this territory, now dotted over with numberless farms and villages and with great towns like Zurich and Geneva, was buried beneath a continuous sheet of snow and ice.

It is not necessary to visit Switzerland to become familiar with the signs left by the ancient ice of the Glacial epoch; they surround us on every hand at home, and are amongst the commonest features of the mountainous parts of our land. Smoothed and striated surfaces, boulder clay and superficial morainic material, testify to the passage of the ice, indicate its direction, afford evidence of its thickness, and determine its boundaries. If we follow the southern boundary of the ice, we shall find that it will take us out of Britain and lead us right across the continent of Europe. After stretching from Kerry to Wexford, and through the Bristol Channel to London, it crosses the sea, continues its course through Antwerp, past Magdeburg, Cracow, Kiev, runs south of Moscow to Kazan, and then terminates at the southern end of the Ural mountains. All that lies to the north of this line—the greater part of the British Isles, Northern Germany, Scandinavia, and almost the whole of European Russia—was buried out of sight beneath a mantle of ice formed by the confluence of many colossal glaciers.

At the same time a large part of North America was

overwhelmed. The great terminal moraine which marks the southern boundary of the ice can be traced with occasional interruptions from Nantucket through Long Island past New York towards the western extremity of Lake Erie, then along a sinuous course in the same direction as the Ohio, down to its confluence with the Mississippi; then it follows the Missouri as far as Kansas City, and beyond runs approximately parallel to that river, but south of it, through Nebraska, Dakota and Montana, and Washington, where it meets the coast north of Columbia river. Within this boundary nearly the half of North America was buried beneath a thick sheet of ice, flowing more or less radiately outwards from a central region situated in and about the region of Hudson Bay.

The co-existence of two continental ice-caps, one on each side of the Atlantic Ocean, must have produced a very profound effect on the climate of the Northern hemisphere; and it can scarcely have originated without a general lowering of the temperature, probably to the extent of 5°C. more or less, over that part of the Northern hemisphere, which lies outside the Tropic of Cancer.

A similar fall of temperature seems to have affected the Southern hemisphere. If we turn to our antipodes we discover obvious signs of the former existence of glaciers in the Kosciusko plateau or Muniong range of New South Wales (lat. $36^{\circ} 22' \text{S.}$, height 7,328 ft.). The snow-fields on the watershed gave birth to glaciers which flowed down the valleys on each side; to the west to a level of at most 6,300 ft., to the east of 5,800 or perhaps 5,500 ft. The largest of these glaciers was only a few hundred feet in thickness and three miles in length.¹ The facts observed in the Kosciusko plateau indicate a former lowering of the snow-line to the extent of 2,200 to 2,700 ft.

In Tasmania, the former existence of Pleistocene glaciers has long been known,² and they point to a lowering of the snow-line to the extent of 4,000 ft.

¹ David, Helms, and Pitman, "Geological Notes on Kosciusko, with special reference to Evidence of Glacial Action," *Proc. Linn. Soc. N.S.W.* 1901, pp. 26-74, plates. This memoir contains a valuable bibliography on the Pleistocene glaciation of the Southern hemisphere.

² T. B. Moore, "Discovery of Glaciation in the Vicinity of Mount Tyndall, etc.," *Papers and Proc. R. Soc. Tasmania* for 1893, pp. 147-9 (1894), and "Notes on Further Proofs of Glaciation at Lower Levels," *Op. cit.* (1896), pp. 73-7. The latest work on the subject is by J. W. Gregory, "A Contribution to the Glacial Geology of Tasmania," *Quart. Journ. Geol. Soc.* 1904, vol. lx. pp. 37-62, plates.

New Zealand differs from Australia and Tasmania, inasmuch as many great glaciers still move down the valleys of its lofty mountains, the Southern Alps, and reach in some cases to within 610 ft. of the existing sea ; but it presents similar evidence of an ancient extension of the ice, and of a lowering of the snow-line by some 3,000 or 4,000 ft.

After a careful consideration of all the facts, Penck concludes that the descent of the snow-line during the glacial epoch was approximately the same in both hemispheres, *i.e.* between 3,000 and 4,000 ft.¹

So far no indications of a Pleistocene glaciation have been observed in South Africa, but the southernmost extremity of the Cape lies north of Mount Kosciusko, the most northerly point in Australia at which glacial markings have been recognised, so that this perhaps is only what might have been expected ; but in South America, which extends farther towards the pole, they are once more manifest ; boulder clay and erratic blocks are widely distributed over the plains of Tierra del Fuego and South Patagonia. After a survey of the evidence Moreno remarks : " In Patagonia an immense ice-sheet extended to the present Atlantic coast, and farther east, during the first ice period ; while, during the second, terminal moraines have been generally left as far as thirty miles north and fifty miles south to the east of the present crest of the Cordillera." ² And Steinmann, in describing his observations, remarks : " Where the ice extended over the plain in a great *mer de glace*, as near the Strait of Magellan, the glacial formations correspond with those of North Germany or the lake region of North America. Where it flowed through deep valleys into the sea, as in the Patagonian archipelago, it repeats the fjord landscape of Norway or Alaska. In the well-watered parts of the Cordillera of Central Patagonia and South Chili, marginal lakes occur, with the same characters as those of the Swiss Alps, bordered by terminal moraines of no great height." ³

If the temperate regions of both hemispheres experienced a lowering of temperature at all approaching 5° C. the tropics

¹ Penck, "Die Eiszeit Australiens," *Zetts. d. Ges f. Erdk. u. Berlin*, 1900, vol. xxxv. pp. 239-86, map.

² F. P. Moreno, *Geogr. Journ.* 1899, vol. xiv. pp. 241-69 and 353-78.

³ Steinmann, "Ueber Diluvium in Sud-America," *Monatsb. d. Deutsch. Geol. Ges.* 1906, No. 7, p. 6 sep. copy.

themselves could scarcely remain unaffected, and we might expect to find some signs of a colder climate even in the torrid zone. Though these signs are to be sought in regions which are difficult of access and rarely visited by skilled observers, yet an increasing body of evidence shows that they actually exist. In South America "traces left by the Ice age extend along the whole mountain chain from Cape Horn (lat. 56° S.) up to the Sierra Nevada de Santa Maria (lat. 11° N.).¹ On Mount Tacora (lat. $17^{\circ} 30'$ S.), the summit of which just reaches the snow-line (19,965 ft.), terminal moraines have been traced down to a level of 13,779 ft., *i.e.* 6,186 ft. below the existing snow-line; Mount Tunari, situated in the more richly watered East Cordillera at about the same latitude ($17^{\circ} 10'$), reaches the snow-line at about 17,000 ft., and its ancient terminal moraines extend down to 9,842 ft., or 8,210 ft. below the snow-line.

The Himalaya and Karakorum, situated it is true outside the tropics, afford concordant testimony; thus in the latest account of these regions we are informed that the existing glaciers, though large and numerous, are but the relics of an older series of ice-flows. The ancient moraines, the perched blocks, and the glaciated surfaces all furnish proofs that the ice in former times covered an area in Asia immensely larger than at present.

On the southern slopes of the Dhauladhar range an old moraine was discovered by the late General MacMahon at the extraordinarily low altitude of 4,700 ft.; and on the Tibetan side of the great Himalayan range the glaciation appears at one time to have been almost universal. No trustworthy observations have yet been made in Central or Northern Tibet; but in Ladak, in Nari Khorsam and in Tsang the vast moraines and the transported blocks, perched high on hillsides far from their parent mass, are indications of the former existence in Southern Tibet of an almost continuous ice-sheet, and of snow-fields and glaciers such as are now to be found in polar regions only.²

The best register however of a former glacial climate within the tropics is afforded by the solitary Mount Kenya (19,500 ft.), which rises only half a degree south of the equator. The glaciers which now flow down its slopes terminate at a height of about 15,400 ft., but the ancient ice extended at least 5,400 ft. lower

¹ Steinmann, *op. cit.*

² Burrard and Hayden, *A Sketch of the Geography and Geology of the Himalaya Mountains and Tibet*, 1907, part iii. p. 192.

down, for a terminal moraine has been observed at 10,000 ft. and erratics have been traced down to 9,800 ft.¹ Similar evidence is afforded by Mount Ruwenzori² and Mount Kilimandjaro.³

Thus, to whatever region we turn, our inquiries elicit the same facts. Alike in Northern Europe and Southern Australia, in the Peruvian Andes or the isolated cones of Central Africa, the evidence points to a considerable lowering of temperature in comparatively recent times, corresponding with the last glacial epoch. Thus the great Ice age clearly deserves its name; it affected the whole of our planet, and can scarcely have failed to influence in a high degree the history of its inhabitants.

Of late years investigations bearing, if possible, even more immediately on our subject, have been directed to the succession of events, or the inner history, of the glacial epoch.

In the British Isles the mountains are so inconsiderable, and the volume of the ice was so great, that secondary effects are lost in the general result, and detailed research is conducted under exceptional difficulties. In the Eastern Alps, on the other hand, both the relief of the ground and the magnitude of the glaciers are such as seem to promise a ready response to fluctuations of temperature, and this under conditions favourable to a permanent record of their effects. Nature seems, indeed, to provide in them a delicate registering thermometer. It was in this way, at least, that they appealed to the sagacity of Prof. Penck,⁴ one of the most distinguished investigators of glacial phenomena at the present day; and it was on the Eastern Alps, therefore, that he first concentrated his attention. Let us follow him into this region. A photograph given in his great work (*loc. cit.*, opp. p. 90) represents one side of the valley of the Steyr. On close examination it will be seen to display a number of parallel terraces, almost horizontal, and running with great regularity in the same direction as the valley. The lowest of these terraces (*w*) forms a broad field through which runs the poplar-bordered road from Steyr to Sierning. It descends by a steep slope, about 50 ft. in height, to the river. Nearly 70 ft.

¹ J. W. Gregory, "The Glacial Geology of Mount Kenia," *Quart. Journ. Geol. Soc.* 1894, vol. l. p. 521.

² J. W. Gregory, "The Geology of Mount Ruwenzori, 1895," *Quart. Journ. Geol. Soc.* 1895, vol. li. p. 676.

³ H. Meyer, *Ostafrikanische Gletscherfahrten*.

⁴ A. Penck and E. Brückner, *Die Alpen im Eiszeitalter*, 8vo, Leipzig, 1901-1908, issued in parts, not yet complete.

above it, the surface of the second terrace (*r*), is seen ; one of the characteristic farmhouses of Upper Austria stands upon it. Immediately behind this follows the third terrace (*m*), and above this again the highest terrace (*g*), which forms a plateau of considerable extent. Such terraces are not confined to the valley of the Steyr ; they are common to many of the great valleys of the Eastern Alps, and are to be found in the Western Alps also, and indeed very generally over Europe, and in all the glaciated regions of the globe.

These terraces can be traced down the valley of the Steyr into the valley of the Enns, and then onwards towards the Danube ; two of them, indeed, the uppermost and lowermost, actually reach the bank of this stream. They can also be traced upwards towards the mountains, extending, with considerable interruptions, over a course of forty or fifty miles. The pits, like our gravel pits at home, which are dug into them here and there, afford an insight into their structure and composition. Entering one of these, we observe beds very much resembling gravel, very coarse, and cleanly washed, made up of pebbles varying from about 2 in. to 6 in. in diameter. On the whole they are rather evenly bedded, sometimes they form oblique layers (false bedding), and they include occasionally lenticular patches of sand or loam. To these deposits the Germans give the name of "shotter," a term we shall find it convenient to adopt. The shotter have evidently been deposited by swiftly running water ; they mark the course of a rapid river.

We may now follow the terraces up the valley, and this time we will select the valley of the Iller. The terraces broaden out to wide sheets, and then become replaced by features of a totally different character. We are now introduced to an irregular assemblage of hills, which extend, not like the terraces, along the valley parallel with its length, but transversely across it, running in a gentle curve convex downwards. They may be overgrown by forests of firs or covered with soft green turf, but natural or artificial sections will somewhere expose their structure. This is very different from that of the river terraces ; instead of rounded pebbles we find angular fragments of rock and an occasional striated boulder, the stones are of all sizes, and of very diverse kinds, fine sand and mud are intermingled with them, and all are thrown together in confusion, with no trace of order or arrangement. These are the characters of a

terminal moraine. Here an ancient glacier of the Iller came to an end.

A question of capital interest now presents itself: what are the relations, if any, between the terrace and the moraine?

The answer to this has been given by Penck, who has shown that the river terrace loses itself in the moraine; the two meet and interdigitate with each other, as shown in the diagram (fig. 1).

Where the glacier gave birth to a river, there the moraine passes into a terrace.

As there are four terraces, so there are four moraines, one to each terrace.

A consideration of these facts leads to very important consequences. In attempting an explanation let us begin with the first or highest terrace. To account for the formation of the

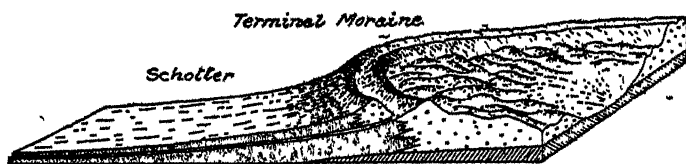


FIG. 1.

thick sheet of shotter it represents, we must assume the existence of a river, so heavily overburdened with detritus, that it had little or no power to erode; it could carry away the material of the moraine, round the angular fragments into well-worn pebbles, and distribute them far and wide over its valley floor, but it could not deepen its channel. Its energy was restricted to building up a sheet of shotter, over a hundred feet in thickness, which stretched from side to side of the river valley. This sheet of shotter represents the first stage in the formation of the terrace (a, fig. 2).

Of the sheet so formed only the first terrace, a mere remnant, a narrow selvage, now exists, lining the side of the valley: the river which previously deposited it has since carried the greater part of it away. It seems natural to assume that the river had acquired a higher degree of activity, probably as a consequence of increased volume and velocity; and its enhanced power is still further shown by the fact that after removing the shotter it was able to wear its way down into the harder rocks

beneath, and has actually deepened its valley. Thus the terrace was cut out during a period of erosion which followed upon a period of deposition (*b*, fig. 2).

The second terrace involves a similar succession of events; it points to a return to the earlier conditions, when the river, powerless to erode, spread out a second sheet of shotter over the newly excavated valley floor (*c*, fig. 2); then came renewed activity, and the second terrace was carved out. The same is true of the third and fourth terraces, and thus we have repeated, time after time, an alternation of periods of deposition and periods of erosion, of overburdening and acceleration. Such are the immediate inferences from the facts.

We must now take a step further, and attempt to account for this alternation of processes.

The interdigitation of the terrace with its moraine shows that the terrace, or rather the sheet of shotter from which it was

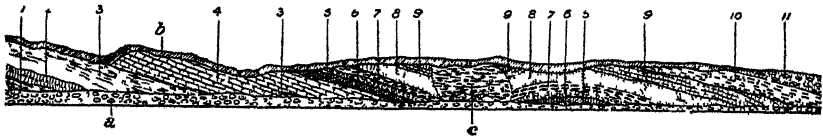


FIG. 2.

carved out, was deposited during an interval when the glacier was comparatively stationary, *i.e.* during the interval in which it built up its terminal moraine. But when a glacier is stationary the amount of water discharged from it is comparatively small, the annual discharge is indeed precisely equal to the annual snowfall by which the glacier is replenished: when the glacier is advancing the discharge is even less. Under these circumstances the resulting river was scarcely larger than the corresponding river which now represents it, and its power to ~~erode~~ carve was at a minimum.

If now we are to endow this river with greater volume and velocity we must assume that the glacier commenced a retreat, or in other words that more ice was melted away from it than was made good by the annual snowfall; and this retreat must have continued for no inconsiderable period—it must have lasted at least as long as was necessary for the sweeping away of the previously deposited shotter and the deepening of the valley.

Thus, if this reasoning be valid, we are led to greatly enlarge

our conception of the glacial epoch: it was evidently no unbroken reign of ice, it was not a single episode, but a repeated alternation of contrasted episodes. There were periods of predominant snowfall, when the ice attained its maximum development, and the rivers were impoverished; and alternating with these were periods of predominant rainfall, when the accumulated ice of centuries melted away, and, adding its volume to the general drainage, gave birth to swollen streams far surpassing in magnitude those with which we are familiar in the existing Alps.

The great ebb and flow of temperature was at least four times

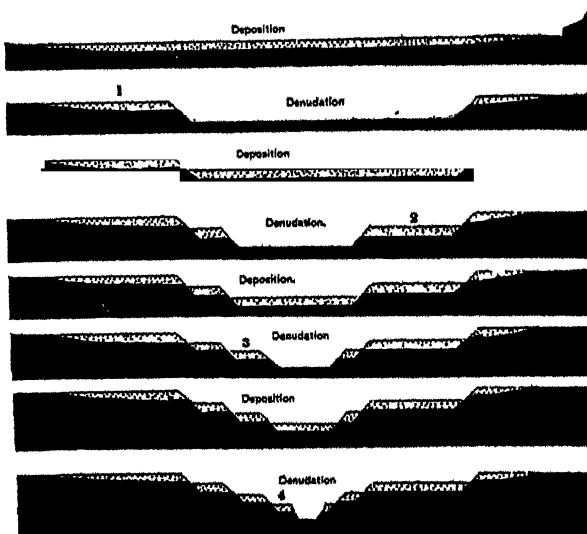


FIG. 3.

repeated; four times have the glaciers enlarged their bounds, and four times have they been driven back into their mountain home.

Such then is the hypothesis which arises from our contemplation of the river terraces; there is much that is attractive about it, and it has the additional advantage of completely explaining the facts, so far as they are known. Yet we must not omit to point out that its author, Prof. Penck, admits it was suggested by the writings of Prof. James Geikie, who in turn was inspired by the theory of Adhemar, as advocated by Croll. At the present day, however, there are few who accept the theory of Adhemar, and consequently the explanation is discredited at its source.

Must we for that reason reject it? By no means: we shall not condemn the prisoner at the bar on account of his pedigree, or because he has been convicted of a first offence. At the same time, in making an unprejudiced inquiry into the case, we shall be more than usually exacting in our demand for proofs.

We will therefore inquire whether there is any independent evidence in favour of these supposed inter-glacial or genial periods. It would seem that there is.

Every one, at least every geologist, who has visited Innsbruck, that delightful starting-place for the mountains, is familiar with the peculiar red stone which is so much used there for building. It comes from some neighbouring quarries situated on the northern slope of the Inn valley, near the village of Hötting. By walking down to the promenade by the side of the river we shall obtain a good general view. The breccia is seen, at a height of about 500 ft. above the bottom of the valley, as an almost horizontal band, several hundred feet in thickness, and very conspicuous owing to the contrast of its reddish colour with the dark blue rock beneath: its course can be plainly traced by the heaps of waste stone thrown out from the workings along its face. Crossing the bridge, a short walk takes us to the quarries. The breccia is then found to consist of fragments for the most part of a dark grey dolomitic limestone cemented together by a reddish marly matrix, and the deposit is such as might result from the consolidation of the débris brought down by a mountain torrent. The rock on which it rests is a dark blue clay containing obviously scratched glacial boulders; it is a true boulder clay, and represents a moraine of the third glacial episode. Since the breccia overlies this, it must be of later date. But higher up, at a height of about 2,500 to 3,000 ft., we encounter a second deposit of boulder clay, a moraine formed during the fourth or last glacial episode. This rests directly upon the smooth surface of the breccia, which must consequently be of earlier date.

Thus the breccia is older than the last glacial episode, and younger than the last but one, and may provisionally be regarded as filling the interval between them—*i.e.* it represents a hypothetical interglacial or genial epoch.

Taken by itself the evidence we have so far offered is not

sufficient to establish so important a conclusion, but fortunately it does not stand alone. The Hötting breccia is fossiliferous, and has yielded a number of leaves and other remains of plants: these fossils are indeed fairly common, and the visitor who should fail to find at least a few examples would be singularly unfortunate. No less than forty-two species have been identified; they include among others the fir (*Pinus sylvestris*), spruce (*Picea* sp.), maple (*Acer pseudo-platanus*), buckthorn (*Rhamnus frangula*), several willows (*Salix nigricans*, *S. glabra*, *S. incana*, *S. triandra*), the wayfaring tree (*Viburnum lantana*), yew (*Taxus baccata*), elm (*Ulmus campestris*), strawberry (*Fragaria vesca*), self-heal (*Prunella vulgaris*), beech (*Fagus silvatica*), and mountain ash (*Sorbus aucuparia*). None of these or of any of the remaining species are of distinctly boreal or alpine type.

Three of the most important plants we have reserved for special mention: they are a new species of buckthorn, *Rhamnus Hoettingensis*, related most closely to *R. latifolia*, now living in the Canary Isles, the box (*Buxus sempervirens*), also a southern species; and most important of all a Rhododendron (*R. ponticum*), which now lives in the Caucasus, five degrees south of the latitude of Innsbruck, and in a climate on the average 3° C. warmer. Taking all the facts into consideration Penck concludes that the climate of Innsbruck in the days of the Hötting breccia was 2° C. higher than it is now: in correspondence with this the snow-line stood 1,000 ft. above its present level, and the Alps, save for the higher peaks, were almost completely denuded of ice and snow.

The region round Hötting thus furnishes us with evidence of revolutions of climate on the grandest scale; the lower boulder clay, representing the third glacial age, witnesses to a time when the snow-line of the Alps had descended 4,000 ft. below its existing level, and the valley of the Inn was filled with ice; the Hötting breccia, representing the third genial age, equally testifies to a time when the ice had disappeared and the mountains had been relieved of their mantle of snow, when also a varied forest growth, thickets of the pontic Rhododendron, and a multitude of flowering annuals covered the bare rocks, and adorned the dreary expanses of boulder clay; the upper boulder clay, representing the fourth and last glacial age, witnesses to a final advance of the ice, when the snow-line again crept down to its previous level, 5,000 ft. below

that of the Hötting interval, and glaciers overflowed the forests of the Inn.

It is fortunate for our argument that the advancing ice did not sweep away and destroy the Hötting breccia, as it has destroyed in all probability a great number of similar deposits. A few other instances of undoubted interglacial beds do, however, exist—notably that of Dürnten, in the neighbourhood of Zurich—and these afford almost equally cogent testimony.

In the light of these facts the imaginary sequence of events suggested by the river terraces acquires a greater appearance of reality, so much so that we may now make use of these features in our subsequent inquiries.

The four terraces are ruled, as it were, across the last page of terrestrial history; they are datum lines, which enable us to divide the Pleistocene or Quaternary epoch into eight ages, the first, second, third, and fourth glacial ages, and a similar succession of genial ages. We are thus provided with a chronological scale to which we can refer the more important events in the early history of the human race.

II

THE dawn of the human race is supposed to belong to a past more remote than the beginning of the great Ice age; yet of the existence of man antecedent to this epoch not a vestige of evidence, forcible enough to compel universal belief, has up to the present time been discovered. Even *Pithecanthropus*, that singular apelike form, which makes the nearest approach to the genus *Homo*, although referred by its discoverer to the Pliocene,¹ has since been asserted on good authority to belong more probably to the Quaternary epoch.²

Thus a problem presents itself at the very outset of our investigation, and as a first step towards its discussion we may commence with an account of the just-mentioned *Pithecanthropus*.

¹ Eugène Dubois, "Einige van Nederlandschen Kant verkregen uitkomsten met betrekking tot de kennis der Kendeng-Fauna (Fauna van Trinil)," *Tijdschrift v.h. K. Nederl. Aardrij. Genoot*, 1907, ser. 2, vol. xxiv. p. 449.

² W. Volz, "Das geologische Alter der *Pithecanthropus*-Schichten bei Trinil, Ost-Java," *Neues Jahrbuch f. Mineral., etc., Festband zur Feier des 100 jährigen Bestehens*, Stuttgart, 1907, p. 256; and Branca, "Vorläufiger Bericht," etc., *Sitzber. d. k. Preussischen Ak. d. Wiss. Berlin*, 1908, p. 261).

On the south flank of the Kenengs, a range of low hills which traverse the eastern extremity of Java, lies a gently undulating series of freshwater and volcanic deposits, formed of consolidated clay, sand, and volcanic lapilli, altogether considerably over 1,000 feet in thickness. They rest on a marine bed of coral limestone about 7 ft. thick, and below this is a bed of clay containing marine shells, all of which are preserved with their valves closed, a sign of sudden death, resulting probably from a volcanic eruption. Such an eruption might have heralded the birth of Lavu-Kukusan, a great twin volcano, more than 10,000 feet in height, and not yet completely extinct, which rises, south of the Kenengs, out of the gently undulating freshwater series.

The river Bengawan, which flows round a great part of the volcano, has cut its way down into the freshwater deposits to a depth of 50 ft., exposing a fine section just at the point where the river touches the Keneng hills, near the village of Trinil. A bed of lapilli at the base is especially rich in Mammalian remains. Vast quantities of bones have been exhumed, affording us, now that their affinities have been determined (E. Dubois, *loc. cit.*), a vivid picture of the life of the time. Various kinds of deer are richly represented: they include the "Sambar," still living in India, the "Kidang," still living in Java, and a new species, *Cervus lyrioceros*. There is also an antelope, *Tetraceros Kroeseni*, allied to an existing Indian form. Next come buffaloes (two species), rhinoceros (two species), a tapir, similar to a living Sumatran form, pigs (two species), hippopotamus, the extinct *Stegodon*, and a true elephant.

Among the Carnivora, the most interesting species is *Felis Groeneveldtii*, said to combine in itself the characters of the lion and the tiger.

There were monkeys, such as *Scenopithecus* and *Macacus*.

The Edentata were represented by a large Pangolin, which attained a length of 8 ft.

In addition to the Mammalia, some birds have been found, such as parrots and marabouts; reptiles also, crocodiles, gavials, and freshwater tortoises; a number of freshwater fish, all belonging to existing species; and a shark, *Carcharias gangeticus*, which points to the proximity of the sea.

Amidst these remains, Dr. Eugène Dubois, who had left Holland for Java with the avowed intention of finding the

"missing link," discovered in September 1891 a molar tooth, the wisdom tooth of *Pithecanthropus erectus*; a month later, between three and four feet away from the tooth, the cranial vault or the skull-cap was found lying in the same bed, and on the same horizon. Work was then suspended on account of the rainy season, but was resumed in May of the following year, and in August the thigh-bone of the left leg was found lying 50 ft. away from the spot where the first tooth was obtained, but still on the same horizon, and finally, in October, another molar tooth, lying 10 ft. away from the skull-cap.

After raising a monument to the memory of this supposed ancestral man,¹ Dr. Dubois returned to Europe, bringing his spoils with him.

The Dutch Government continued the excavations at Trinil after Dr. Dubois' departure, but beyond an additional molar tooth nothing of importance was found. Recently, however, the district has been visited by several investigators. Prof. Klaatsch explored the neighbourhood in search of implements such as might have been made by *Pithecanthropus*, but he was unable to examine the bed from which it was obtained, as this was submerged to a depth of 3 ft. by the swollen waters of the Bengawan. Prof. Volz of Breslau (*loc. cit.*) has made a special geological study of the district. But the most important of recent expeditions is that conducted by Madame Selenka, which left Berlin in 1906.² The naturalist who assisted her, M. Carthaus, is said to have found that many of the bones of the animals already mentioned have been split longitudinally, as though to extract the marrow; some have been polished and fashioned into weapons, and others, as well as fragments of wood, have been burnt by fire. Indeed, it is asserted that a hearth has been discovered, with ashes, and fritted fragments of clay and sand. A complete description of the remains of *Pithecanthropus* has been published by Dr. Dubois,³ and they

¹ It stands on the edge of a cliff, overlooking the last resting-spot of *Pithecanthropus* (previous to his removal) and has served as a useful guide to subsequent investigators.

² "Die Selenka-Expedition nach Trinil," *Globus*, vol. xciii. p. 58, 1908. For an interesting summary see "The Age of *Pithecanthropus*," by J. Deniker, *L'Anthropologie*, 1908, tom. xix. p. 260.

³ E. Dubois, "*Pithecanthropus erectus*, eine menschenähnliche Uebergangsform aus Java," Batavia, 1894, 4to, p. 44; and "*Pithecanthropus erectus*, transitional form between Man and the Apes," *Sci. Trans. R. Dublin Soc.* 1898, vol. vi. pp. 1-18.

have been studied by almost all the leading anatomists in Europe. All are agreed that they indicate an animal bearing a close resemblance to men and apes, but beyond this opinions are no longer in harmony: some regard *Pithecanthropus* as an ape with certain human characters, others as a man with evident simian characters; others again, and in particular Dr. Dubois himself, regard it as a connecting-link, standing midway between man and the higher apes. The suggestion has even been made that the remains are those of a microcephalic idiot, or again of a monster begotten of human and simian parents.

Disregarding those opinions which have little of probability to recommend them, let us review the question in broad outline.

That which distinguishes man from all the beasts of the field is the power and complexity of his mind,¹ and whether the brain be a dream of the mind or the mind a dream of the brain, the two are certainly associated in a manner as close as it is inexplicable. Thus the chief interest in the Trinil fossil attaches to the skull-cap or brain-pan. As regards both its general form, and all those niceties of modelling which require the trained eye of an anatomist for their appreciation, this is certainly more simian than human. Prof. J. D. Cunningham recognises many features which remind him of the gibbon; Prof. Schwalbe sees more resemblance to the chimpanzee; and, though neither of these authorities is inclined to push his comparisons too far, yet they are both agreed in asserting that the affinities indicated by the form of the skull-cap are on the side of the anthropoid ape rather than man. The forehead of *Pithecanthropus* is even more receding than that of the chimpanzee, the occiput scarcely less so, and the altitudinal index, *i.e.* the ratio of the height of the skull-cap to its length, is almost the same in both. The value of this index in *Pithecanthropus* is 34·2: in the lowest known human cranium it rises to 40·4, while in the average European it is no less than 52.²

¹ In these days of triumphant athleticism this sounds like a startling paradox: even in our Universities the power to kick a football through a goal or to row a boat to victory would almost certainly be considered a criterion of at least equal value.

² Less importance is now to be attached to these numbers, since it has been shown that the base-line from which the measurements are made is not trustworthy. See Sollas "On the Cranial and Facial Characters of the Neandertal Race," *Phil. Trans.* 1907, ser. B, vol. 199, p. 294.

The matrix with which the skull-cap was filled when it was first brought over to Europe has since been carefully removed, so as to expose the interior, thus rendering it possible to obtain a plaster cast which represents approximately the form of the brain.

Next to mind, speech is the distinctive faculty of man ; some thinkers have even given this the first place. But the power of speech resides in a particular fold of the brain, the lower frontal lobe, occupying the region known as Broca's area. Fortunately this region can be identified in the case just alluded to ; its area has been measured, and is said to be twice as great as in the anthropoid apes, but only half as large as in man.¹ Thus in this one respect *Pithecanthropus* may be truly regarded as a middle term. If further we are justified in arguing from organ to function, then we may fairly conclude that this primitive precursor of the human race had already acquired the rudiments of vocal speech.

We have left the most important character to the last : this is the size of the skull-cap, or rather its capacity for containing brains. According to the latest measurements of Dr. Dubois the cranial cavity has a volume of 850 cubic centimetres. We must not omit to point out, however, that this can only be taken as a more or less probable estimate : the skull is far too incomplete for exact measurement. Dubois' first estimate was 1,000 c.c.

The cranial capacity of the higher apes is not known to exceed 600 c.c., and that of a healthy human being never falls, so far as existing observations extend, below 880 c.c. ;² the mean of these two numbers is 740, and this should be the capacity in cubic centimetres of a form standing midway between the lowest man and the highest ape ; but, as we have seen, this limit is already exceeded in *Pithecanthropus*, even to the extent of 110 c.c., and thus, judged by a character which we must regard as of the very highest importance, *Pithecanthropus* must be included within the limits of the human family. In the long ancestral series which extends upwards from the apes to man he has mounted far more than half way, and only a few steps of the

¹ E. Dubois, "Remarks on the Brain-cast of *Pithecanthropus erectus*," *Journ. Anat. and Phys.* 1899, vol. xxxiii. pp. 273-6.

² Out of 904 Tyrolese skulls one was found with this minimum capacity. It is asserted to be perfectly normal in other respects.—F. Tappener, *Zeits. f. Ethnologie*, 1899, xxxi. p. 304.

long ascent remain to separate him from the species *Homo sapiens*, essential man.

By a strange phraseology *Pithecanthropus* is often spoken of as "the missing link," as though instead of many thousands there were only one intermediate form between man and his less highly endowed relations. Still more serious misconceptions are involved in the use of this term, as we shall see later.

We have now passed in brief review the chief features of the skull-cap: as to the molar teeth, they are large and coarse, such as are appropriate to the skull: the premolar has not yet been described.

Especial interest attaches to the femur or thigh-bone: it is distinctly human, and belongs without doubt to an animal which walked erect. But with the erect attitude is correlated the differentiation of the extremities into hands and feet, one of the most important of human characteristics.

As we have seen, the femur and skull-cap were not found close together, but separated by an interval of 50 feet; there is thus no absolute proof that they belonged to the same animal, though in view of the extraordinary rarity both of human and simian fossil remains it would be very astonishing if they had not. This is very generally admitted, and thus the animal they represent has been fittingly designated *Pithecanthropus erectus*—the ape-like man, who walked erect. Attempts have been made to portray him in the flesh, but these exercises of the imagination are of no scientific value. Judging from the length of the femur (455 mm.) his stature is supposed to have been 1700 mm., or about the same as that of an average Englishman.

The operations of the mind doubtless find their noblest expression in the language of speech, yet they also express themselves eloquently in the achievements of the hand. The works of man's hand are his embodied thoughts, and it is by their means, much more than by the fragmentary and rare remains of his corporeal frame, that we shall now be assisted in our efforts to decipher the earliest signs of his unwritten story.

We have seen that the most primitive member of the human family hitherto discovered was in existence during the early Pleistocene epoch, and if we may regard *Pithecanthropus* as representing the prevailing race which peopled the Southern hemisphere at that time we shall scarcely expect to find evidence

of intelligent workmanship in deposits of much greater antiquity, certainly not in the remote Oligocene or Miocene.

On the other hand, the assumption that Pithecanthropus marks the highest level of contemporary human evolution will certainly not pass unquestioned, unless supported by collateral evidence, of which at present there is none to be found.

The anthropoid apes make their earliest known appearance in the middle Miocene (*Dryopithecus*, *Pliopithecus*) in company with true Old World monkeys (*Oreopithecus*): the lower monkeys and lemurs had long previously been in existence. It may be noted in passing that at this date the evolution of the horse had proceeded as far as the stage represented by the three-toed horse, *Anchitherium*, and that of the elephant as far as the mastodon.

If man has descended in a direct line from the anthropoid apes, then we might expect to find some trace of a primitive ancestor in the Miocene; if, on the other hand, as some suppose, both the anthropoid apes and man are descended from a common stock, then we might look for a still earlier appearance of some form in the direct line of human descent. To discuss on *a priori* grounds these rival hypotheses does not lead to enlightenment—possibilities and probabilities may be urged on each side; but in either case it is clear that palæontology affords no reason either for or against the conclusion that primitive man was in existence during Miocene times, nor on the other hand that he was not. We thus gain an impartial standpoint from which to review the statements of those who claim to have discovered evidence of tool-making animals in various epochs of the Tertiary era.

Thenay.—The first investigator in this field was the Abbé Bourgeois,¹ who in 1867 discovered a number of broken flints in beds of upper Oligocene age near Thenay, a village situated south of Orleans in the department of Loir-et-Cher. M. Bourgeois was of opinion that they had been shaped by man, and he observed a peculiar crackling of the surface which he attributed to the action of fire. Distinguished investigators, d'Omalus, d'Halloy, de Quatrefages, and G. de Mortillet, not to mention others, shared the opinion that they showed evidence of intelligent design;

¹ Bourgeois, "Sur les Silex considérés comme portant les marques d'un travail humain découverts dans le terrain Miocène de Thenay," *Congr. d'Anthr.* Brux. 1872, p. 81.

equally distinguished authorities, Virchow, Desor, and Fraas, maintained the contrary. De Mortillet believed that they had been made not by man himself, but by a semi-human precursor which he named *Homosimius Bourgeoisii*.

Otta.—Fragments of quartzite and flint were next found by Carlo Ribeiro¹ in lacustrine beds of upper Miocene age at Otta, a village not far from Madrid. These have been attributed by G. de Mortillet to another species of *Homosimius*: *H. Ribeiroi*.

Puy Courny.—The upper Miocene of Puy Courny, near d'Aurillac, in the department of Cantal, Auvergne, has also furnished numerous flints of supposed human workmanship. They were discovered by J. B. Rames in 1877, and from that time to

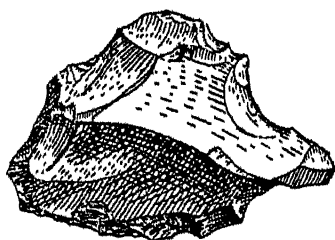


FIG. 4.



FIG. 5.



this the locality has proved a battle-field for contending opinions, the combatants on each side being equally confident in the strength of their cause. In this case there is no question as to the palæontological horizon from which the flints have been obtained: it is unquestionably upper Miocene, and has furnished remains of extinct mammals, such as *Dinotherium giganteum*, Kaup; *Mastodon longirostris*, Kaup; *Rhinoceros Schleiermacheri*, Kaup; *Hipparion gracile*, Kaup. The sole point in dispute is whether the flints have or have not been fashioned by man or a precursor of man. The accompanying illustration (figs. 4, 5, and 6) represents one of an artefact origin.

The veteran anthropologist de Quatrefages asserts that if these forms had been met with in Pleistocene deposits no one

¹ C. Ribeiro, "Descrição de alguns sílex a quartzites lascados encontrados nas camadas dos terrenos terciário e quaternário," Lisboa, 1871.

would have doubted their artificial nature. Prof. Max Verworn, after a close examination of the flints obtained in his excavations, concludes that 24 per cent. show "indubitable signs" of workmanship, and in the adjacent locality of Puy de Boudieu even 30 per cent.; about half are classed as "doubtful," and only 15 to 20 per cent. as of inorganic origin. Like Prof. A. Rutot, who thinks he can recognise the special purposes for which these fragments were used, classifying them into hammers, anvils, scrapers, burins, missiles, Max Verworn regards them as proof of a fairly well differentiated culture, and he concludes that at the close of the Miocene epoch the valleys of the Cantal were peopled by beings who were already familiar with the art of splitting flints by blows and the formation of implements by comparatively fine marginal chipping under the action of skilfully produced rebounds. G. de Mortillet, while agreeing as to the artificial character of these forms, attributes them to the hypothetical *Homosimius*, and distinguishes a new species—*H. Ramesii*.

On the other hand, M. Marcellin Buole is unable to perceive any signs of intelligent workmanship, and the latest investigator of Puy Corny, Dr. Lucien Mayet, concludes that natural agents, such as variations of temperature, torrential rushes of water, subsidence of the deposits, and no doubt others of which we are ignorant, have played the principal part in the formation of the "coliths" of Cantal.

When experts are thus at variance nothing remains for the layman but to preserve an open mind.

Burma.—In 1894 Fritz Noetling¹ recorded the occurrence of curiously shaped flints in beds of lower Pliocene age near Yenang-yung in Burma: he suggested that they may have been chipped into shape by man. Prof. T. Rupert Jones,² after an examination of the only specimen which has yet been figured, asserts that "there can be no doubt as to the artificial dressing of this flake." Mr. R. D. Oldham³ has shown, however, that the flakes were found lying on an exposed surface, and that it is very doubtful, therefore, whether they are of the age attributed to them.

East Runton.—In the late Pliocene deposit, known as the Forest Bed of Norfolk, some flints were found by Mr. W. J. L.

¹ F. Noetling, "On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma," *Rec. Geol. Surv. India*, 1894, xxvii. pp. 101-3, pl.

² T. Rupert Jones, "Miocene Man in India," *Nat. Sci.* 1894, v. p. 345.

³ R. D. Oldham, "The Alleged Miocene Man in Burma," *Nat. Sci.*, 1895, vii. p. 201.

Abbott¹ in 1897, and these present many features suggestive of human workmanship. Sir John Evans, who has examined them, expresses himself with great reserve. The specimen "No. 4 may or may not be artificial, and the same may be said of No. 3, with even greater probability of its having been made by man."

Boncelles.—Last of all, M. E. de Munck² and M. A. Rutol³ have discovered abundant chipped flint flakes, which they regard as representing an "eolithic industry," in pebble beds of supposed middle Oligocene age. The true nature of these flints and the geological horizon on which they occur are still *sub judice*.

We have now reached the end of this summary, and find ourselves precisely where we were, having obtained no evidence either for or against the existence of man in times previous to the great Ice age.

The subject bristles with difficulties naturally inherent to it. The finished flint implement, which we shall meet with later in Pleistocene deposits, is a work of art—every touch tells of intelligent design; but it was not achieved, we may feel sure, by a sudden inspiration: if we adopt the uniformitarian tenets of the day we shall be led to suppose that it was a product of slow growth, the issue of a long series of preceding stages. The first implements to be used by a creature of dawning intellect would be those that lie ready to hand; a pebble seems specially designed for a missile, yet it will also serve admirably for a hammer, and a broken flint is by no means a bad knife.

But broken flints are not always to be found when wanted; in that case they may be made at will by the simple process of striking one against another; and when this momentous experiment has been made the equally momentous discovery follows that the resulting fragments possess an edge of almost unrivalled keenness. The first step has now been taken, the stimulus of discovery soon leads to others; directly the tyro takes to hammering flints he begins to perceive with what tractable material he has to deal, he learns its habits and tricks of fracturing, and so passes on to shape it into forms which he has already framed in his mind as suitable to meet his ends. These will bear every evidence of design; the pebble which he

¹ W. J. L. Abbott, "Worked Flints from the Cromer Forest Bed," *Nat. Sci.* 1897, x. p. 89.

² E. de Munck, "Les alluvions à éolithes de la terrasse supérieure de la vallée de l'Ourthe," *Bull. Soc. Belge de géol.* xxi. 1907. Pr.-verb.

³ A. Rutol, "Un grave Problème," *Bull. Soc. Belge de géol.* xx., 1907. Mem.

flung from his hand preserves no record of its flight ; but between these two lie many intermediate stages which might puzzle the wisest to decide whether they have been formed by accident or intent. Nature graduates nicely into art, and we have no talisman at present to tell us where one ends and the other begins.

Finally there is nothing in the nature of this hypothetical evolution to inform us whether it was rapid or spread over a long period of time.

Hence it happens that anthropologists are divided into two opposing, almost hostile, camps.

We may usefully supplement our account of the actual evidence by a brief review of the arguments which have been used on each side.

It may be remarked at the outset that those who advocate the human origin of many "eolithic" forms too often seem to confuse the possibility with the probability that a particular stone may have been used by man. For instance, the Andaman islanders obtain sharply pointed fragments of flint by heating the stone in a fire and then plunging it into water ; they make use of these fragments for drilling holes in bits of shell which then serve as a sort of beads ; the drills are soon blunted by use, and are then thrown on to the kitchen midden, where along with other refuse they accumulate in thousands.¹ This fact is cited as evidence of the human workmanship of the flints of Thenay.²

But flints of the Andaman islanders carry very little proof of their origin in themselves ; our knowledge of them is founded first on direct observation of the process by which they were made, and next on their association with other signs of human occupation in the rubbish heaps of the village. If they occurred without this collateral evidence, strewn through a mass of in-organically-shaped flints, we should in all probability be unable to establish their true nature. But it is a curious inversion of reasoning which would argue that since certain fragments of stone, devoid of any sign by which they could be recognised as of human workmanship, have nevertheless been made and used by man, therefore certain other fragments, equally devoid, in the opinion of their opponents, of such signs, must also have been used by man.

¹ Man, "On the Aboriginal Inhabitants of the Andaman Islands," *Journ. Anthropol. Inst.*, vol. vii. p. 244.

² Engerrand, "Six Acons de Préhistoire," Bruxelles, 1905, p. 50.

A second example of the same argument is afforded by Prof. Engerrand,¹ who writes: "One of the most striking confirmations of the theory advocated by M. Rutot is the discovery of primitive tribes which are still in the stage of simply using stone. Such are the Seris Indians, inhabitants of the islands of Tiburon and Sonora, described by McGee.

"The arms of the Seris are stones collected from the beach, and serve without preparation as the hammers which they use in everyday life. They carry these stones in their fights and employ them in combats man to man."

Without questioning the completeness of this story, we may yet feel some doubt as to whether the Seris Indian would be able to recognise to-morrow the pebble he fought with to-day, and still more whether an observer coming on the scene some thousands of years after would succeed in discovering in such missiles the marks by which they could be distinguished from those lying on the beach.

Another argument, frequently employed by the advocates of the theory of intelligent design, is that we know of no other way of accounting for the form of these flints; but this is always a very dangerous logic: we are not yet so intimately familiar with all the processes of nature as to be able to proceed by a method of exclusion. Indeed, it may be alleged that forms very similar to those in dispute are almost certainly produced by river and torrent action, as well as by pressure beneath the soil or by the action of the frost. It is possible to pick out of almost any gravel pit containing angular material any number of chipped forms, and among them some which are difficult to distinguish from supposed "eoliths": but these make very little impression on the true believer, for he has always two resources open to him—either the natural form may be distinguished from the artificial by some slight difference in detail, which is only perceptible to a gifted eye, or it may be boldly claimed as a true artefact.

In this connection some observations made within the last few years at Guerville possess a certain importance. At this spot, some two kilometres south-east of Mantec, on the right bank of the Seine, is a cement works from which every form of "eolith" is said to be produced in great numbers daily as a

¹ *Tom. cit.*, p. 97, and W. J. MacGee, "Germe d'une Industrie de la Pierre en Amérique," *Bull. Soc. d'Anthr.*, Paris, 1902, p. 82.

by-product. The cement is made by intimately mixing chalk and clay; but the chalk contains a number of flint nodules, some of which find their way into the mill. This is a circular basin in which the chalk and clay are stirred together along with water by a revolving rake, five metres in diameter, moving with a velocity at its outer edge of four metres per second, or the same velocity as the Rhône in flood. The flints are thus exposed to a succession of violent impacts during a space of twenty-six hours, the time required to secure adequate mixture. When the operation is ended the mud is drawn off and the flints remain at the bottom of the vat. Some have been converted into true pebbles; others, according to M. Marcellin Boule, present all the characteristic features of eoliths—the same bulbs of percussion, pointed ends, curvilinear notches, and edges broken by “retouches.” They are indeed “of extraordinary perfection, and appear to be the result of fine workmanship.”

Obviously it is not by such arguments as those we have just considered that the inquiry will be advanced. Much more to the point is the work of M. Rutot, who, steadfastly pursuing the comparative method, endeavours to trace the resemblances between these doubtful forms and those which are generally admitted to be of palæolithic workmanship.

It is impossible not to admire the courage and perspicacity which M. Rutot brings to his task, but proof must depend on the degree of similarity which he is able to discover, and further, a likeness which will produce conviction in one mind will fail to do so in another. I am myself deeply indebted to M. Rutot for the kindness and patience with which he unfolded his evidence before me in several lengthy demonstrations. If I refrain from offering an opinion it is because it would be of no consequence to the argument, since it would but increase by an insignificant unit the large number of partisans arrayed on either side; and till there is a general consensus of opinion, one way or another, the conclusion of this chapter may be sustained, which is that no sufficient evidence has so far been obtained of the existence of man or his immediate precursors in any epoch anterior to the Pleistocene. To this negative we may add a positive result, that the lowest term of the human series yet discovered is represented by *Pithecanthropus*, and dates from some part of the Pleistocene epoch.

REVIEWS

The Practical Study of Malaria and other Blood Parasites. By J. W. W. STEPHENS, M.D. Cantab., D.P.H., and S. R. CHRISTOPHERS, M.B. Vict., I.M.S. Third Edition. [Pp. 414.] (London, 1908: Published for the University Press of Liverpool by Williams & Norgate; price 12s. 6d.)

THE third edition of *The Practical Study of Malaria and other Blood Parasites*, by Christophers and Stephens, differs in some important respects from the previous edition. The knowledge of parasitology, and of the intermediate hosts of parasites, has steadily increased, and a book of this nature has necessarily, therefore, to include fresh subjects with each edition.

The authors, in this instance, have been able to make this increase without adding materially to the bulk of the book, by excluding subjects previously dealt with. Of these exclusions the most important is the chapter dealing with Filaria. These parasites are important clinically, and the recognition and differential diagnosis of the embryos found in the blood are, in many tropical diseases, of considerable importance, whilst the methods of examination required, though simple, are of value. It is a difficult matter to decide on such a question, but it is to be hoped that the authors may find it practicable to restore the chapter in future editions. The actual space saved, however, is considerable, as the necessity for dealing with many groups of the Culicidae is diminished, and attention can be confined to the most important group, the Anophelina.

The arrangement of the book is much the same as in the previous editions. Malaria is considered fully. Not only are the parasites described in detail, but also the changes they induce in the corpuscles in which they live. A special feature is an elaborate analysis of the leucocytes in malaria considered in a later chapter (Chap. XXII.).

The mosquitoes are considered in detail as regards some of the anophelines only and *Stegomyia calopus*. As these mosquitoes are the carriers of malaria and yellow fever respectively, a knowledge of them is necessarily required in the study of these diseases, and in the space of a book of this kind it is impossible to include the enormous number of species, or even genera, that are now known. Theobald's classification is followed. A very valuable part of the work is the description of the larvæ of the anophelines. As the different species vary greatly in the part they play in the propagation of malaria, it is of great practical importance to be able to diagnose the species from the larvæ instead of by the tedious and often difficult process of waiting till they have hatched out.

The dissection and anatomy of the mosquito are fully described, as well as the characters of the zygotes and sporozoites of the malaria parasites.

A useful chapter is that on the method of making a "malarial survey" of a station, illustrating well the importance of determining species and the proportion of mosquitoes infected with malaria. The authors prefer for this purpose the determination of the sporozoites rather than of zygotes and capsules of zygotes, which, however, have the advantage that they can be recognised more certainly by a beginner.

The chapter on blackwater fever is an excellent one, and indicates lines for future work,

An extensive list of the Hæmocytozoa in lower animals is given, but in many cases the description is too brief to be of practical value.

The chapter on ticks has been enlarged, and there is a good account of the internal anatomy and the method of dissection. Trypanosomes, Leishman Donovan bodies, and spirochætes are considered in detail as regards human species, whilst notes, usually brief, are given of those occurring in other animals.

The chapter on blood-sucking flies is too short to be of much practical value as an aid to diagnosis to any but an expert.

The book is a valuable one and should prove of much practical value to those working in the tropics. It is well printed and profusely illustrated; some of the illustrations are good.

C. W. DANIELLS.

The Law of Hospitals, Infirmaries, Dispensaries, and other Kindred Institutions, whether Voluntary or Rate-Supported. By ARTHUR TURNOUR MURRAY, Barrister-at-Law, B.A. Oxon. [Pp. xviii + 288.] (London, 1908: John Murray; price 10s. 6d. net.)

MR. MURRAY'S object in compiling this book was, as his preface states, "to bring together into one volume the many legal questions which affect all kinds of medical institutions." In spite of the difficulty of his task, Mr. Murray has achieved a success upon which he may well be congratulated. The book should be most useful, not only to legal practitioners, but also—and perhaps more especially—to hospital secretaries and officials. It is well and simply arranged under thirty-two alphabetical heads, with a good index, and a comprehensive list of cases and statutes. Under these thirty-two heads it may fairly be said that every branch of the law as relating to hospitals is dealt with—in some cases more fully than in others—as for instance the law of infectious disease hospitals and death in hospitals. There is an interesting classification of hospitals in the introduction; other chapters deal with subjects—to select a few out of many—such as accounts, contracts, gifts, insurance (dealing particularly with the Workmen's Compensation Act, 1907), vivisection, vaccination, land. Much of the law on these subjects is statutory, and where this is so, Mr. Murray has succeeded in grouping the relevant provisions in a manner which is at once intelligible and convenient. In a necessarily short review it is impossible to deal with any particular portion of the book at any length, but we may call special attention to the two headings which Mr. Murray has treated perhaps more fully than any others; namely, death in hospital (pp. 37-60) and infectious disease hospitals (pp. 76-114). The former deals (*inter alia*) with dissection, and the provisions of the Anatomy Act, 1832, applicable thereto. Inquests, cremation, post-mortem examinations, are also treated in this chapter. The latter contains the statutes affecting infectious disease both inside and outside London. The relevant sections of the Isolation Hospitals Acts are set out, and an account is given of the Metropolitan District Asylums in London, and their management. The law relating to midwives, and the settlement of bastards, and duties as to registration of birth are contained in the chapter on "Lying-in Hospitals." The law of negligence as it affects hospitals is a question of vital importance to them, and to it Mr. Murray devotes a separate chapter. On the question of the effect of s. 1 (a) of the Public Authorities Protection Act, 1893, on suits brought under Lord Campbell's Act (p. 183), a reference might usefully have been made to the case of *Williams v. Mersey Dock and Harbour Board* (1905, 1 K.B. 804). A chapter is devoted to meetings (pp. 157-72) which states concisely the formalities necessary in the calling of a meeting,

and the conduct of business thereat. The investment of hospital funds (pp. 121-3), and the acquisition and sale of lands by hospitals (pp. 124-38) form the subject of two other useful chapters.

It is hoped that this short summary may show in some measure that Mr. Murray's book contains information which should render it invaluable to all those interested in the legal and economic aspect of our hospitals.

H. BEEZLEY.

Systematic Anatomy of the Dicotyledons: A Handbook for Laboratories of Pure and Applied Botany. By DR. HANS SOLEREDER. Translated by L. A. Boodle, F.L.S., and F. R. Fritsch, D.Sc., Ph.D., F.L.S. Revised by D. H. Scott, M.A., LL.D., Ph.D., F.R.S. Vol. I., Introduction, Polypetalæ, Gamopetalæ. With 153 figures in the Text. Oxford: At the Clarendon Press, 1908; price 24s. net.

THE appearance of a translation of Solereder's great work on the anatomy of the Dicotyledons is to be regarded as one of the most noteworthy events in recent English botanical literature. The book forms one of the remarkable series of foreign classics, done into English and published by the Oxford University Press, which the younger generation of English-speaking botanists have come to regard as among their greatest guides, philosophers, and friends. And no fault will be found if yet another opportunity be taken of recording the debt of gratitude which they and all botanists owe to the Clarendon Press for rendering so ready of access the masterpieces of German work.

It is fitting that the work of the revision and translation of one of the greatest examples of foreign anatomical work should have been delegated to the group of men mentioned in the title-page quoted above. One of the most notable features of the growth of British Botany during the last one or two decades has been the large amount of attention paid to anatomy—more especially that branch of it dealing with the vascular system—by what has come to be recognised as a definite British school of anatomists. Prominent in this school are the botanists to whom the present volume is due, and the association of the names of Dr. Scott, Mr. Boodle, and Dr. Fritsch with this work is an all-sufficient guarantee that both the spirit and the letter of Professor Solereder's masterpiece have been rendered the fullest justice of which English botany and the English language are capable.

The intention and scope of Solereder's *Systematische Anatomie der Dicotyledonen* is well known to botanists, but the work is of that specialised character which would render it practically unknown to laymen. For this reason it has been thought desirable to refer at some length to the circumstances which rendered the appearance of such a work a desideratum. It may be said that practically the whole of the efforts of early botanical science were directed towards the naming, identification, and recording the descriptions of the plants which at that time were pouring into the European herbaria from travellers and collectors in all parts of the world; and of allotting to the plants received a place in the scheme of classification which, for the time being, was generally held to be the most satisfactory. It must even be admitted that the earlier schemes of classification aimed not so much at establishing the natural relationships of plants, as at affording a convenient series of compartments into which newly named and described plants could be laid. At this period comparatively little attention was paid to internal anatomy, nor, in the absence of reliable nomenclature, would work in this direction have had much scientific value. But, with the growth of such a knowledge, a mass of anatomical observations gradually came into being, and the first period may be

said to have culminated in the publication, during the latter half of last century, of De Bary's *Vergleichende Anatomie*. The results accomplished had to a large extent been ignored by purely systematic botanists, and there can be little doubt that in many herbaria at the present day the anatomical and physiological aspects of the questions at issue receive comparatively little consideration. But although such is the case, the fact must not be lost sight of that the importance, or at any rate the desirability of the use of anatomical characters in taxonomic work had been very early recognised by some systematists, among whom may be mentioned Linnæus himself. Moreover, certain anatomical evidence has been long employed, consciously or unconsciously as such, by systematists in arriving at their conclusions, and in this connection we need only mention the fundamental anatomical difference implied in the use of the terms "exogen" and "endogen" in distinguishing the two great classes of ordinary flowering plants; the recognised importance of the presence or absence of endosperm; and the occurrence in the plant of resin-containing glands. And it must be admitted that, in its essentials, the question is one of degree; for while it is obviously convenient to base the description and classification of plants upon characters which are more or less readily visible to the naked eye, *i.e.* upon general external characters, it by no means follows that characters involving the use of the microscope and histological methods are of any less value for taxonomic purposes. It was a realisation of this truth which led Radlkofer in 1883 to express his opinion that, in coping with the problems of systematic botany, "the next hundred years will be devoted to the anatomical method"; and Solereder has informed us that the prediction of Radlkofer was directly responsible for the compilation of this work towards which his own researches have contributed so largely.

The book deals with the Dicotyledons alone among the Phanerogams, and the classification adopted is that of Bentham and Hooker. Up to the present, the first volume only has been completed by the translators, and this comprises Solereder's Introduction, and the survey of the polypetalous and gamopetalous orders; the author's valuable Concluding Remarks, in which he summarises the case for the anatomical method, have thus not yet appeared. The simple plan of the work renders its contents very easy of access. The description of each natural order opens with a general view of its essential anatomical features, followed by a detailed account of the structure of the plant body, arranged under the headings of "Structure of the Leaf" and "Structure of the Axis." It will be readily understood that the amplitude of the descriptions of the different orders varies greatly, depending entirely upon the amount of work which has been done upon them by anatomists. Further, the structure of the vegetative organs alone is considered, and of them practically the whole of the work is concerned with the leaf and stem, the former having been found to possess, in the character of its stomata, hairy coverings, and secreting sacs, features of great taxonomic value. A chronological series of references to literature is given at the end of the description of each order, and in the English edition special efforts have been made to bring the bibliography down to the most recent times.

In reviewing the claims of the anatomists, the question naturally presents itself as to how far the method advocated has been successful in taxonomic research. In his Introduction Solereder emphasises the extreme caution which must be exercised in basing opinions upon anatomical (endomorphie) as opposed to external (exomorphie) characters. He points out the distinctions which must be drawn between characters which can only be explained as hereditary characters, the origin of which is lost, and those which are regarded as a result of adaptation to

special modes of life, e.g. the aquatic, alpine, and xerophytic conditions. Further, insistence is made upon the necessity for studying the *constancy* of characters within a species—an unsettling admission neutralised to a large extent, however, by the reflection that variations of this type would appear never to exceed certain fixed limits. Bearing these qualifications in mind, there can be little doubt that the anatomical method—still but a young branch of Botany—will accomplish much. Much, indeed, has already been done; and the important place ascribed to anatomical considerations in *Die natürlichen Pflanzenfamilien* of Engler and Prantl—a monumental work affording probably the most natural scheme of classification yet produced—is sufficient evidence that the value of these criteria is realised by some of the greatest systematists. There is, however, another aspect of the question, for Solereder's work is a "Handbook for Laboratories of Pure and Applied Botany." The anatomical method has for a long time been employed in pharmacognosy and in work dealing with the detection of food adulterants, but in this country, at any rate, comparatively little systematic work has been done on these lines; and the application of botanical histology to the identification of the innumerable other economic products of the vegetable kingdom may be said to be practically non-existent. But there is strong evidence that this state of things will not long be permitted to remain. In this country, as well as abroad—notably in Germany and the United States—increased attention is being given to the question of the adulteration of foodstuffs and drugs. For the detection of the impurities, exact methods of microscopical examination are necessary, and that the resources of botanical science are fully capable of dealing with the problem is evidenced by the fact that the recent revision of the legislation dealing with food and drug adulteration in Germany has resulted in the appearance of exact scientific treatises—notably the *Pharmakognostisches Praktikum* of Koch and Gilg—intended for the training of those to be entrusted with the maintenance of the purity of these articles; and in the United States the increased attention given to similar matters has been accompanied by the translation into English of Hanausek's *Lehrbuch der technischen Mikroskopie*, and the appearance of related works of Winton and others. The book forming the subject of the present notice has long been recognised as one of the most valuable aids in this branch of work, and the appearance of an English edition will be warmly welcomed by all economic botanists in this country as an invaluable help in elucidating the difficult problems with which they have to deal.

In finally surveying the work of the anatomical school, there can be little doubt that it should be cordially received by the systematists. The ultimate aim of systematic botany is the formulation of a scheme of classification which shall illustrate—so far as is humanly possible—the true natural relationships of plants, fossil and recent. For the complete attainment of this end evidence of every kind must be brought to bear—evidence of the fossil record, of morphology, anatomy, physiology, and teratology. Moreover, the majority of botanists now realise that the evidence of the sister sciences must not be neglected, notably that of chemistry. Solereder himself strongly emphasises the assistance which could be rendered by micro-chemistry, but deplors the imperfection of existing methods and the great difficulties of technique which render this line of work comparatively sterile. From this point of view probably one of the most satisfactory pieces of systematic work yet carried out is that of Baker and Smith in *A Research on the Eucalypts, especially in regard to their Essential Oils* (1902), in which the authors—a botanist and a chemist working conjointly—were led to revise the usually accepted classification of an important genus, and to formulate another based upon habit, characters of timber and bark, and the chemical and physical

properties of the oils characteristic of the plants. And it must be recorded that the species thus defined are found to be practically constant in botanical and chemical features under all conditions of geographical range.

Finally, no claim is made for the anatomical method that it supersedes that based on external characters. "The anatomical method is only an auxiliary one, although it is of great importance. Properly employed . . . it is of the greatest value both as a means of confirmation of results already obtained, and for the further elaboration of the Natural System."

S. E. CHANDLER.

The Chemistry of the Diazo-Compounds. By JOHN CANNELL CAIN. [x + 170 pp.] (London, Edward Arnold, 1908.)

THIS monograph on the chemistry of the diazo-compounds is an excellent example of a type of work too rarely seen in English chemical literature. The author, who is fully conversant with the technical as well as the purely scientific applications of the diazo-compounds, has succeeded in imparting a considerable amount of first-hand information, together with a comprehensive survey of the investigations of all the other workers in this field. The work appears in the jubilee year of the diazo-compounds, for it is now fifty years since Johann Peter Griess prepared the first diazo-derivative, by treating picramic acid with cold nitrous acid. It is therefore a fitting and convenient time for taking stock of the data which have accumulated during half a century of steady progress.

Two-thirds of the treatise deals with matters of fact, the remaining third being devoted to a concise exposition of the various views which have been held in regard to the constitution of diazo-compounds. Throughout both these sections the author has given so full a bibliography that practically all important statements are accompanied by references (in footnotes) to the original sources of information.

As the chief scientific value of the diazo-compounds is their employment in the synthesis of derivatives of aromatic hydrocarbons, the chapters relating to the preparation and reactions of these substances are a mine of information to the chemist who proposes to use the diazo reaction. Some of the recipes are given in full with all necessary working details.

The chapters devoted to the consideration of the theoretical speculations will be read with special interest by all students of the subject, for, with the exception of the reviewer's Brit. Assoc. report on the aromatic diazo-compounds (1902), no other résumé of these extremely intricate and controversial matters has yet appeared in English. In this connection it should be stated that the treatise now under review deals with aliphatic as well as aromatic diazo-derivatives.

The German monographs on diazo-compounds, which have been written by Hantzsch and his pupil Eibner, are somewhat biassed in favour of the stereochemical theories of diazo-configuration advocated by the former. The author, while doing justice to the ingenuity and persistency displayed by Hantzsch in elaborating and defending his views, has endeavoured to present the case for those who, like Bamberger, prefer an explanation based on structural differences of constitution. The author has himself put forward a theory of the constitution of diazonium salts, which has elucidated certain obscure phenomena, and has served as a stimulus for fresh researches.

The value of this very readable book as a work of reference is increased by the inclusion of both name and subject indexes.

G. T. MORGAN.

Mines and Minerals of the British Empire: Being a Description of the Historical, Physical, and Industrial Features of the Principal Centres of Mineral Production in the British Dominions beyond the Seas. [Pp. xx + 403.] Price 15s. net. (London: Edward Arnold, 1908.)

THIS is an eminently readable account of the mineral industry of the Empire outside these islands. The author, who is a resident in Johannesburg, has visited most of the localities which are dealt with at any length, and has made the most of his opportunities. The geological relations of the deposits are usually well described, and where there are rival theories as to their origin he gives a fair statement of both sides of the controversy. Interesting information is furnished with regard to mining methods and the economic conditions under which the work is carried on, and the varying character of the labour employed in different cases is well brought out. It is a pity that he was unable to see the Mysore Gold Mines, for it would have been interesting to have had his views on the work of the Indian and Italian miners, who were suggested as possible alternatives to Chinese labour in South Africa—the former being rejected on account of the conditions laid down by the Government of India, and the latter because it was not considered seemly that the Kaffirs should see white men engaged in manual labour. While not disguising his disapproval of the democratic principles that govern the New Zealand mining policy, he recognises, with the fairness that is characteristic of the whole book, the success with which it has been attended.

The great subject of Australian Mining is somewhat unequally and inadequately treated. Tasmanian fields, however, obtain full recognition, though there is a curious slip in the statement on page 107, that the conglomerate series belongs to "an earlier geologic period than the unconformable underlying schists." In Western Australia the information given is almost confined to Coolgardie and Kalgoorlie. Other mining centres are almost ignored, and the tin and tantalite of Wodgina and Greenbushes are not mentioned. Other omissions include the coal, lignite, tin, antimony, copper, and tungsten of Victoria; the opals, sapphires, molybdenum, tungsten, and monazite of Queensland; the Burmese jade-stone, the salt mines of the Punjab, the graphite of Travancore, and the mangak of Trinidad. The thorianite of Ceylon is not, as stated, of merely scientific interest, but a mineral of considerable commercial value.

The subject of geological survey administration is dealt with in an appendix, but no adequate reference is made to the important mineral surveys carried out by well-known geologists and mineral experts, officers of the Imperial Institute, in Ceylon, Northern Nigeria, Southern Nigeria, Nyasaland, and Uganda. Not only is a careful examination of the mining resources of the Colony made on the spot, but typical specimens are forwarded to the Scientific and Technical Department of the Imperial Institute, examined and reported on, and finally added to the great public collections of colonial products. While the commercial aspects are necessarily mainly regarded, scientific questions that arise in the course of the work are by no means neglected.

J. W. EVANS.

THE DESTRUCTION OF WOOD BY FUNGI

By A. H. REGINALD BULLER, D.Sc., Ph.D.

(Professor of Botany at the University of Manitoba)

MILLIONS of tons of wood are produced every year in the forests of the world. Observation, however, tells us that the sum-total of wood upon the surface of the earth remains fairly constant from year to year and from century to century. We must conclude, therefore, that there are destructive agencies at work by which millions of tons of wood are destroyed annually. Regarded in this light the problem of what these destructive agencies are, and how they act, becomes of general scientific and economic interest.

The balance between the amount of wood formed by trees and that destroyed in a given period of time, such as a century, has not always been kept. An excess in production over destruction led to the formation of the Coal Measures. There is no reason to suppose that the assimilatory activity of trees during the Carboniferous Period was materially greater than now, if similar climates be taken into account. The accumulation of wood seems to have been due to the geological conditions having been unfavourable to the destructive agencies, and in particular to fungi. The dead parts of trees, which are deposited on the floors of most forests at the present day, are kept constantly moist or wet, so that the cell-walls contain imbibed water, whilst the cell-lumina are chiefly filled with air. Under these conditions the wood-destroying fungi flourish, and cause even the giants of the forest to rot and disappear within a comparatively few years after death. The exact manner in which coal accumulated during the Carboniferous Period has been a matter of dispute, but there can be no doubt that the forest trees grew for the most part in low-lying, swampy regions. Palæobotanists have also found fungus hyphæ in fossil wood, so that it seems probable that wood-destroying fungi were quite common in those early times. Possibly some day the fruit-bodies of *Fomes* or other species of *Polyporci*, attached to tree-trunks, will be discovered, but so far nothing of the kind has

been brought to light. The fallen leaves, twigs, branches, and tree-trunks of the Coal Measure forests seem to have been quickly plunged into free water, and thus to have become thoroughly soaked. They further appear to have been carried by streams into salt lagoons¹ or lakes, and there to have been finally deposited. At the present day such conditions are found to be very unfavourable to the wood-destroying fungi. The mycelium of these organisms does not seem able to develop in wood which has its cell-lumina filled with free water. Wooden piles, submerged beneath either fresh or salt water, remain long, if not entirely, protected from fungi. Thus the piles in the lake dwellings of Switzerland and Ireland have lasted for centuries. When the piles of Old London Bridge² were taken up they were found to be sound after six hundred and fifty years of use. Complete saturation of wood with water, which now preserves timber from the inroads of fungi, was in all probability just as unfavourable to these organisms in the Carboniferous Period. When this is admitted it is easier to understand the accumulation of the materials which have formed the coal seams.

Among the agents at present at work in destroying wood, animals will first be shortly considered. When compared with plants, however, they play but a subordinate part in the great process.

Almost all the higher animals leave wood untouched, if for no other reason, because its mechanical resistance defies their teeth. A few rodents have the habit of gnawing wood, partly for the purpose of making burrows in which to live or hide, and partly in order to obtain food. The enormous annual destruction of wood, including standing timber, by fire through the agency of man is, of course, a matter of common knowledge. Charred wood, found by palæobotanists in rocks which were formed before man had been evolved, indicates that forest fires have originated in the past by lightning or spontaneous combustion.

Certain Crustacea of the group Malacostraca, e.g. *Chelura terebrans*,³ bore into wood piles and thus weaken them. Some Molluscs of the group Lamellibranchiata, e.g. *Teredo navalis*, the "ship-worm," have the same habit. By boring into piles the

¹ Geikie, *Text-book of Geology*, 3rd ed., p. 805.

² G. S. Boulger, *Wood*, 1902, p. 254.

³ R. Hertwig, *Lehrbuch d. Zoologie*, 4^{te} Aufl. p. 386.

animal has been the cause of the breaking of certain dams in Holland,¹ with the loss of many human lives.

It is well known that many insects bore into wood. According to Drummond,² in certain parts of tropical Africa not a stick falls to the ground but is immediately converted into powder by the activity of the Termites or White Ants. The true Ants, too, sometimes make their living-rooms in wood.

Numerous species of Coleoptera inhabit wood. One of the best known is *Anobium striatum*, which often destroys furniture, in which it bores circular holes. This insect flourishes in air-dry timber, where it is practically free from all competitors. No fungus could grow under such conditions. It seems probable that *Anobium* and its allies obtain the water necessary for the structure of their tissues by splitting up carbohydrates such as cellulose and starch. The beetles live upon the wood into which they bore. The exact manner in which they act upon the lignified membranes still requires elucidation, but doubtless they digest the starch and protoplasm contained in the medullary rays and wood parenchyma by acting upon them with appropriate enzymes.

We may now consider the vegetable organisms which destroy wood. Bacteria are the cause of the breaking down of so many organic substances that it might well be expected that they are also concerned in the destruction of wood. As yet, however, no wood-destroying bacteria have been isolated. Bacteria which destroy unlignified membranes, consisting of cellulose, are very wide-spread in nature, and, as Omelianski³ has shown, can be obtained from any soil containing humus, or pond where vegetable matter is decomposing. Nevertheless, when cellulose cell-walls have become lignified, they long resist the action of bacteria. These organisms are unable to penetrate into solid wood, and thus have little chance to act upon it until, owing to the attacks of fungi, it has reached the last stages of destruction.

The chief agency at work in destroying wood is undoubtedly the Fungi. Of these the groups concerned are the Ascomycetes and the Basidiomycetes.

In order to clear the way for subsequent remarks, it may be

¹ R. Hertwig, *loc. cit.* p. 328 ; *vide* also Hedley, *Austral. Ass. Adv. Sci.* 1901, p. 237.

² H. Drummond, *Tropical Africa*.

³ Omelianski, *Ref. in Chem. Centralbl.* 1898, Bd. I. p. 269.

stated that every wood-destroying fungus consists of two parts, a vegetative and a reproductive. The vegetative is known as the *mycelium*, which is very much branched and made up of very fine threads or *hyphæ*. These penetrate in all directions into the cells of the wood and cause it to go rotten. Such decayed wood is often known popularly as "touchwood." The reproductive part of the fungus develops on the outside of the rotting wood. It is known as a sporophore, or *fruit-body*. It is devoted exclusively to the production and liberation of spores, and is developed entirely at the expense of the food obtained by the mycelium hidden in the wood. Some fruit-bodies are very large and form brackets or hoof-shaped masses of various hues. When projecting from tree-trunks or logs they may attract the attention of the least observant.

The Ascomycetes rarely do much damage to the wood in bulk, such as tree-trunks or thick branches. They are numerous, however, upon twigs and the surface of wood. They require a comparatively small amount of food to produce their small fruit-bodies.

The Basidiomycetes are the most active destroyers of wood. The lower forms have comparatively simple fruit-bodies, and are often found on small branches. The higher forms, however, have more complex and, as a rule, much more massive fruit-bodies. When wood is the food, this is obtained by acting with enzymes upon the substance of thick branches and tree-trunks. In this manner the Basidiomycetes remove most of the wood which falls to the floor of our forests.

It is admitted that if timber be kept perfectly dry, or submerged beneath water, it will resist decay for an indefinite period. It was formerly thought that alternate exposure to air and water was sufficient *per se* to bring about the decay of wood. There is, however, no evidence that such is the case in the absence of living organisms.

The causes which lead to the decay of wood were elucidated toward the latter end of the nineteenth century. Just three decades have passed since the publication of Robert Hartig's classical researches.

Theodore Hartig,¹ father of Robert Hartig and the discoverer of sieve-tubes and aleurone grains, was the first to investigate the destruction of wood by fungi in a scientific manner, and

¹ T. Hartig, Berlin.

he published the results of his work in 1833 in a paper entitled "Abhandlung über die Verwandlung der polycotyledonischen Pflanzenzelle in Pilz- und Schwamm-Gebilde und der daraus hervorgehenden sogenannten Fäulniss des Holzes." Hartig's views of the Red Rot of trees were as follows: As a tree passes a certain age the functions of its parts begin to be lost, bringing about the decay of the wood. This, however, may happen earlier, in consequence of unfavourable external conditions. The first step in decay is the breaking up of the contents or membranes of the wood-cells into tiny balls or monads. These monads gradually form rows and fuse, thus becoming converted into fungus hyphæ. These hyphæ can then grow and infect wood which is sound and cause it to rot. Hartig called the fungus by the somewhat fanciful name of *Nyctomyces* (Nachtfaser), as an indication that the hyphæ originated in deepest darkness. Although Hartig shared the views of his time, and explained the presence of the fungus mycelium by spontaneous generation, he discovered facts which made him hesitate in this conclusion. Having observed the fruit-body of a *Polyporus* upon the outside of a rotten tree in which he could make out the *Nyctomyces* hyphæ, he asked himself the question whether the fruit-body had not been produced by the *Nyctomyces*. He failed to find the connecting hyphæ, the presence of which, he said, would entirely alter his views upon the origin of the *Nyctomyces*. Hartig's work, however, was of considerable value, for it added to our knowledge the fact that the decay of wood is usually connected with the presence of fungus hyphæ.

The origin of the hyphæ in decaying wood from spores produced by fruit-bodies became clear from the researches of de Bary, Tulasne, and others, upon plant-diseases.

Schlacht¹ investigated the changes brought about by fungi in dead plant-cells. He observed the disappearance of starch, protoplasm, and cell-walls, and paid particular attention to the passage of hyphæ through cell-walls and to their corrosion. Owing to the absence of fruit-bodies Schlacht failed to identify the fungi which he saw.

In 1866, Willkomm, in his *Microscopische Feinde des Waldes*, gave an account of some investigations upon Red Rot and White Rot. He observed different kinds of hyphæ in rotting

¹ Hermann Schlacht, *Jahrbucher f. wiss. Bot.* 1863, Bd. 3, p. 442.

wood, but failed to connect them with the fruit-bodies of Basidiomycetes. He added little that was new concerning the decay of wood. It is evident that the phenomenon was still very incompletely understood.

In 1878 appeared Robert Hartig's¹ work called *Zersetzungserscheinungen des Holzes*. It was provided with many excellent illustrations, and threw a flood of light upon the decay of wood. The fungi concerned and their method of action were described in detail. The account included *Trametes radiciperda*, *T. pini*, *Polyporus fulvus* (*Hartigii*), *P. vaporarius*, *P. mollis*, *P. borealis*, and *Agaricus melleus* upon the Coniferae, and *Hydnum diversidens*, *Thelephora perdis* (*Stereum frustulosum*), *Polyporus sulphureus*, *P. igniarius*, *P. dryadeus*, and *Stereum hirsutum* upon the Oak. Hartig showed that each fungus has a specific action upon wood, so that it is often possible by macroscopic inspection of a piece of rotten wood to determine the species of fungus concerned in its decay, even in the absence of fruit-bodies. Great attention was paid to the corrosion of cell-walls and their dissolution. The disappearance of starch-grains and protoplasm was observed. Chemical analyses showed that, as wood decays, in many cases it becomes relatively richer in carbon. It was found that some fungi, such as the Dry Rot Fungus, remove the cellulose from the cell-walls, leaving behind a skeleton of friable wood-gum, whilst others, e.g. *Stereum hirsutum*, delignify and decolorise the lignified membranes so that the cell-walls come to respond readily to the microchemical reactions for pure cellulose. From these and many other observations it became clear that the chemistry of wood decay differs much with the different species of fungi concerned.

Several other publications of Hartig² were more or less devoted to the decay of wood. The chief is his monograph on *Merulius lacrimans*,³ the so-called Dry Rot fungus of our houses. It may be pointed out that the term "Dry Rot" is a misnomer, for the fungus can only exist in damp timber, such as occurs in ill-ventilated cellars. Hartig showed that the fungus is able to creep some feet over brickwork, etc., from one beam or board to another, by developing curious strands containing specialised

¹ Berlin, 1878.

² *Wichtige Krankheiten der Waldbäume*, Berlin, 1874; *Unters. aus d. Forstbot. Institut zu München*, 1883, Bd. 3; *Pflanzenkrankheiten*, Berlin, 1900.

³ *Der echte Hausschwamm (Merulius lacrimans)*, Berlin, 1885.

conducting elements. The penetration of the hyphæ in the wood from tracheide to tracheide through tiny holes made in the walls was observed. Microchemical tests were found to indicate that juices excreted by the fungus bring about the removal of the cellulose from the wood cell-walls. With this chemical change is associated the mechanical weakening of the rotting woodwork. An account was given of the structure of the well-known fruit-bodies, and germination of the spores was described for the first time. Much of Hartig's work was devoted directly to the economic aspect of Dry Rot, and his book contains a number of suggestions upon the best means of protecting house-timber from the ravages of the fungus.

Marshall Ward¹ wrote a note upon "Penicillium as a wood-destroying Fungus." In a paper "On the Biology of *Stereum hirsutum*" he² described pure cultures of the fungus from the spore to the fruit-body, and gave an account of the changes it produces in wood.

A pure culture of a wood-destroying fungus, *Collybia velutipes*, was made by Constantin and Matruchot,³ and later also by Biffen.⁴ In a more recent paper by Falck,⁵ a description with photographs is given of pure cultures of *Collybia velutipes*, *Hypholoma fasciculare* and *Phlæbia merismoides*. The fruit-bodies were grown from oidia, which were produced by the breaking up of the mycelium grown in pure cultures from spores.

In a paper "On the Biology of *Bulgaria polymorpha*, Wett.," Biffen⁶ has described the delignification of the cell-walls and removal of the middle lamellæ from the wood of the Oak by the fungus.

Czapek⁷ has made an important contribution to our knowledge of the enzymes of wood-destroying fungi. He has extracted an enzyme from *Merulius lacrimans*, which he states brings about the delignification of the wood. He regards a lignified cell-wall as consisting, in part at least, of an ester of cellulose and hadromal,⁸ which can be split up into its con-

¹ Marshall Ward, *Ann. of Botany*, 1898, vol. 12.

² *Ibid.*, *Phil. Trans. Roy. Soc.* 1897, vol. 189.

³ Constantin et Matruchot, *Compt. Rend.*, 1894, t. 119.

⁴ Biffen, *Journ. Linn. Soc.*, 1899, vol. 34.

⁵ Richard Falck, *Beitr. z. Biol. d. Pflanzen*, Cohn, 1902, Bd. 8.

⁶ Biffen, *Ann. of Botany*, 1901, vol. 15.

⁷ Czapek, *Ber. d. D. Bot. Gesell.*, 1899, Bd. 18.

⁸ *Ibid.*, *Zeitschr. f. physiologische Chemie*, 1899, Bd. 27.

stituents by the enzyme which he has named *hadromase*. The cellulose set free can then be broken down by cytase. The chemistry of lignified cell-walls is still to a large extent a matter of conjecture. Only when it becomes more fully cleared up can we expect to understand many of the chemical changes taking place in rotting wood.

In America, von Schrenk¹ has published a number of papers upon the destruction of wood and the diseases of forest trees. His work is of practical as well as of scientific interest.

In 1905 the writer² gave an account of the destruction of wooden paving-blocks by *Lentinus lepideus*, Fr. Fig. 1 shows the relationship of the fungus hyphæ to the wood-cells.

The following is a summary of the conclusions arrived at :

1. The destruction of a large number of paving-blocks, made of pinewood, in the city of Birmingham, is being brought about by *Lentinus lepideus*, a fungus belonging to the Agaricini. Considerable repairs to the pavement are thereby necessitated.

2. Single blocks or small groups of blocks at intervals in the streets go completely rotten, so that one can break up the wood with the fingers. The streets affected become unduly bumpy. In wet weather puddles collect above places where rotten blocks are.

3. A number of rotting blocks, obtained from time to time from the streets, were placed in a large damp chamber. In the course of a few weeks fruit-bodies of *Lentinus lepideus* appeared upon them.

4. The spores remain unchanged in distilled water and tap-water, but germinate readily in Pasteur's Fluid and in beef-gelatine. They also germinate in decoctions of horsedung and of pinewood.

5. The pavement is probably infected by spores after the

¹ Hermann von Schrenk, U.S. Dep. of Agric., Veg. Phys. and Path., 1900 Bull. 25, "Some Diseases of New England Conifers"; *ibid.*, 1900, Bull. 21, "Two Diseases of Red Cedar"; Twelfth Ann. Rept. Missouri Bot. Gardens, 1902; *Journ. Western Soc. Engineers*, 1901; "Factors which cause the Decay of Wood," Contribution 19, Shaw School of Botany, St. Louis; "Fungus Diseases of Forest Trees," *Yearbook*, U.S. Dep. of Agric., 1900; U.S. Dep. of Agric., Plant Industry, Bull. 14, 1902, "The Decay of Timber and Methods of Preventing it"; *ibid.*, Bull. 32, 1903, "A Disease of the White Ash caused by *Polyporus fraxinophilus*"; *ibid.*, Bull. 36, 1903, "The Bluing and Red Rot of the Western Yellow Pine, etc."

² A. H. R. Buller, "The Destruction of Paving-Blocks by the Fungus *Lentinus lepideus*, Fr.," *Journ. of Economic Biology*, 1905, vol. 1.

blocks have been laid down. The mycelium often grows from a rotten block to the neighbouring sound ones. No fruit-bodies are produced in the streets owing to the traffic.

6. The wood is rotted by *Lentinus lepideus* in very much the same manner as by *Merulius lacrimans* (the Dry Rot fungus). It

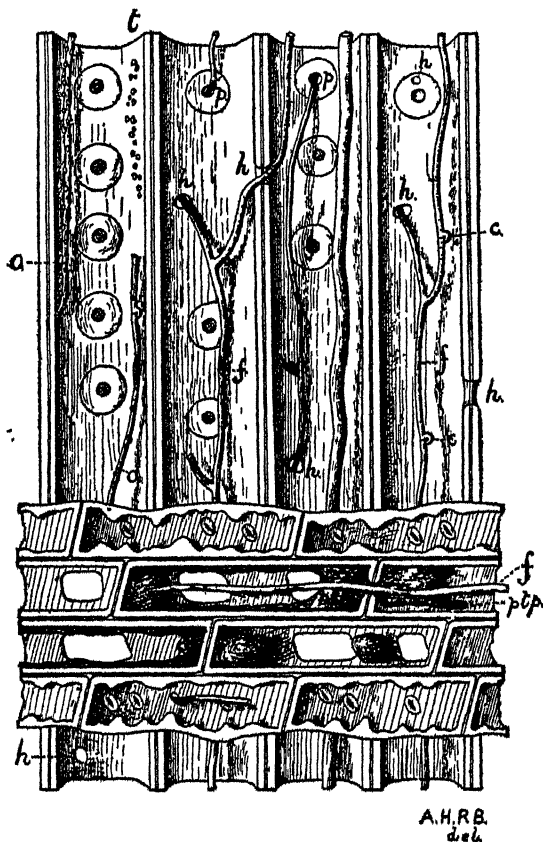


FIG. 1.

Mag. 450. Radial-longitudinal section of wood of Pine from a rotten paving-block. A medullary ray is represented crossing four tracheides. The hyphae, *h*, of *Lentinus lepideus* are to be seen in the cell lumina. They are sometimes branched, and are provided with clamp connections, *c*. They make their way from cell to cell through bordered pits, *p*, or bore holes, *h*, through the walls. Some hyphae are coated with crystals of calcium oxalate, *o*. At *t* the hypha has disappeared and a trail of crystals has been left behind. Cell contents, *ppp*, still remain in some of the medullary ray cells.

becomes red and spongy when wet; it shrinks and cracks considerably on drying, and is then very brittle and friable. Cellulose is removed from the cell-walls and hadromal left behind.

7. The paving-blocks, used in the pavements referred to, were dipped in creosote before use. Had they been fully impregnated with that substance, the ravages of *Lentinus lepidus* or any other wood-destroying fungus would have been prevented.

Much still remains to be done in elucidating the relationship of wood-destroying fungi to living trees. Some years ago Marshall Ward called attention to the fact that we knew practically nothing about *Polyporus squamosus*, one of the commonest Polyporei in England. The writer undertook to investigate its life-history, and in 1906 published a fully illustrated paper on "The Biology of *Polyporus squamosus*, Huds., a Timber-destroying Fungus."¹ A short account of the plant will now be given.

Polyporus squamosus, the Great Scaly Polyporus or Saddle-back Fungus, is one of the best known of the tree-destroying fungi. Its large ochraceous fruit-bodies, checkered with brown scales above, are frequently to be seen projecting as conspicuous brackets, either singly or in groups, from branches or the trunks of trees in woods, parks, and gardens. The fungus destroys a large number of ornamental trees, and has therefore excited the interest of many who have the care of them. It is not only common in Europe, but is also found in the United States. Whilst residing in Munich the writer had frequent opportunity of observing the fruit-bodies. In 1901 a series of fine old Horse Chestnuts and Sycamores were cut down in the Hof-garten owing to the majority having suffered from fungus attacks. In three cases fruit-bodies indicated the presence of *Polyporus squamosus*. During July 1901 four of the Red Chestnut trees in front of the Wittelbacher Palast were bearing fruit-bodies simultaneously. In the Englischer Garten two Wych Elms and an Ash were found in a similar condition. In England the fungus is equally common. Upon old trees in the suburbs of Birmingham it was seen on the Beech, Sycamore, Wych Elm, and *Pirus vestita*. The fungus was also noticed at Sutton Coldfield, Havant, Banbury, Oxford, and on the Elms in the Backs at Cambridge. *Polyporus squamosus* has never yet been found on Pine, Larch, or other Coniferæ. Otherwise it is not particular in its choice of hosts, for various authors record it upon the following trees: Sycamore, various species of Maple,

¹ A. H. R. Buller, *Journ. of Economic Botany*, 1906, vol. i. pp. 101-138.

Horse Chestnut, Pear, Mountain Ash, Oak, Elm, Ash, Willow, Birch, Lime, Walnut, and Hazel.

The fruit-bodies of *Polyporus squamosus* are the largest among British fungi. Few other species growing on trees form such conspicuous objects. During the summer of 1905 the writer gathered a perfect specimen 2 ft. 2 in. across, and having a perimeter of 6 ft. 3 in. It weighed approximately 6½ lb. As a rule, however, the fruit-bodies are not so large. Usually they occur several together, and the largest are then about a foot in diameter.

The fruit-bodies are annual. They are produced from May till September. In the Midlands of England they are especially abundant in July. If developing in warm, sunny weather, insects soon find them out, causing them to become infested

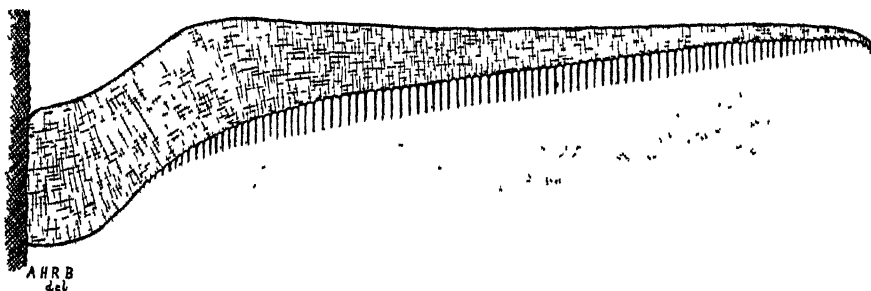


FIG. 2.

Mag. ½. Section through a fruit-body of *Polyporus squamosus* showing the hymenial tubes from which clouds of spores are falling.

with larvæ often before reaching maturity. They may thus become putrescent and fall within a few days or weeks after being formed. This is particularly the case with large specimens. A fruit-body is nothing more than an organ of reproduction. Its function is to produce, liberate, and entrust to the winds millions of microscopic spores. On the under side of a fruit-body of *P. squamosus* are developed some thousands of narrow hymenial tubes, which have their axes vertically placed and their mouths or pores freely opening below (Fig. 2). The spores are produced by the hymenium with which the tubes are lined. It was found that the spores are discharged with violence from their sterigmata. They are shot out horizontally to a distance of about 0.1 mm. They then make a sharp turn and fall vertically downwards in the tubes, without danger of

touching and adhering to the sides. They thus reach the open air in safety. Their rate of fall in still air was carefully measured, and found to be about 1 mm. per second. Spore-discharge lasts from five days to a fortnight. It is a continuous process, taking place night and day in dry and wet weather. The spores are produced in vast abundance. With a Leitz-Wetzlar counting apparatus it was calculated that each hymenial tube in one instance had produced nearly 2,000,000. The large fruit-body, already referred to, may well have given rise to 10,000,000,000. From another fruit-body the spores were liberated at the rate of about a million a minute.

Notwithstanding the activity of spore-discharge from the fruit-bodies of most Hymenomycetes, as a rule the falling spores cannot be seen with the naked eye. *Polyporus squamosus* proved to be a rare exception. The visibility of the discharge in this instance appears to be due partly to the great rate of spore-liberation from the large fruit-bodies and partly to the large size and colourless nature of the spores themselves. A fruit-body, some 10 in. across, was grown on a log in an experimental greenhouse where the air was very quiet. Under these conditions clouds of spores, resembling steam or the finest white smoke, were observed falling continuously for thirteen days. The wreaths and curls of spores drifted slowly away from the fruit-bodies, and became gradually lost at a distance of about two yards. Wreaths of spores were distinctly seen on a black background at a distance of 30 ft.

The fruit-bodies of some wood-destroying fungi—e.g. *Fomes igniarius*—are perennial. They are then very hard and of woody or corky consistency, and produce new hymenial tubes and spores each summer. They may continue their activity for ten or more years. On the other hand, the comparatively soft fruit-bodies of *Polyporus squamosus* are annual. They live but a few weeks altogether, and soon die after the spores have been discharged.

The spores of *Polyporus squamosus* are dispersed by the wind. As their rate of fall in still air is only about 1 mm. per second, it will be readily conceived that slight breezes are able to carry them very long distances before they settle. The chances of a spore falling upon a suitable wound surface of a tree, where germination and infection may take place, must be extraordinarily slight.

Polyporus squamosus is usually a wound parasite. Its spores in all probability germinate upon the wound surfaces of broken branches, where the mycelium can make its way directly into the wood. There is no evidence to show that an uninjured tree can become infected. The bark of trees appears to be fungus-proof. It entirely resists penetration of any germ-tubes from without. So far as the writer's experience has gone, fruit-bodies of the fungus are always to be found actually upon or near obvious wound surfaces, where the wood has been laid bare, so that germ-tubes could have made an entrance directly into it. As trees get older they become more and more liable to have wood surfaces exposed, owing to damage by wind, snow, lightning, frost, etc. The older trees become, therefore, the more likely they are to be infected by wood-destroying fungi. In parks and gardens, for various reasons, branches are frequently cut from Horse Chestnut trees, Sycamores, Beeches, etc., and thus admirable wound surfaces prepared by which *P. squamosus*, or any other wood-destroying fungus, may make its way into the trunk. The experience of the writer goes to show that in many cases infection has almost certainly taken place in this way, fruit-bodies having subsequently appeared on the wound surfaces. When branches are sawn off valuable ornamental trees, the exposed surfaces left ought to be at once creosoted or treated with some other antiseptic, to prevent the germination of spores. If this is not done there is always considerable risk that wood-destroying fungi will enter the trees and gradually cause their decay. When a mycelium has once established itself in a tree-trunk, it cannot be killed or its growth checked without injury to the tree itself.

When the mycelium of *Polyporus squamosus* has entered a large branch, guided by the wood cells, it makes its way gradually to the centre of the tree-trunk, and then spreads upwards and downwards in it, causing it to rot from within outwards. The fungus spreads slowly to the periphery, thus reducing every year the amount of sound wood. The conduction of water up the stem appears to be slowly interfered with. At any rate one notices a gradual dying away of the branches of an infected tree. Finally the whole tree may succumb, but, if it is large, its death is often long delayed. Fruit-bodies may appear annually for ten or more years upon an Elm or Sycamore, whilst it is still living. The wood, how-

ever, becomes more and more extensively rotted as time goes on, until at length an unusual storm may cause the trunk to snap in two. When a tree infected by *Polyporus squamosus* has died, the fungus can still continue its annual production of fruit-bodies. It is then a pure saprophyte. With the tree-destroying fungi, which live in the wood, there is usually no hard-and-fast line between parasitism and saprophytism.

The mycelium of *Polyporus squamosus* makes its way into the cell-lumina of the infected wood. It does this by boring small holes in the lignified cell-walls. The ends of the hyphæ probably excrete an enzyme or enzymes capable of rendering soluble the hard membranes. Through holes thus made the fungus hyphæ readily pass from cell to cell. In course of time the least lignified cell-walls disappear entirely. The autumn wood, the medullary rays, and the vessels with the sheath of cells around them are the most lignified elements in Sycamore wood, and here they last longest, but become more and more riddled with holes (Fig. 3). At length the wood becomes reduced to a skeleton of its former self. The decay is known as White Rot, because the wood turns much lighter in colour under the action of the fungus. Starch, protoplasm, and cellulose are gradually removed. They are probably rendered soluble by means of appropriate enzymes. This view is supported by an investigation into the enzymes of the fruit-bodies.¹ The following were found to occur in the expressed juice: laccase, tyrosinase, amylase, emulsin, protease, lipase, rennetase, and "coagulase," whereas negative results were obtained in the tests for pectase, maltase, invertase, trehalase, and cytase. However, a study of the destruction of wood by the fungus affords evidence that the mycelium produces cytase and possibly hadromase. Altogether,



AMR
etc.

FIG. 3.

Mag. 300. A fibre isolated by Schulze's maceration mixture from the rotten wood of a Sycamore. The holes in the cell-wall were made by the mycelium of *Polyporus squamosus*.

¹ A. H. R. Buller, "The Enzymes of *Polyporus squamosus*," *Ann. of Botany*, vol. 20, 1906, pp. 49-59.

timbers, telegraph and telephone poles, and, indeed, structural timbers of all kinds.

A few years ago the United States Department of Agriculture commissioned Hermann von Schrenk to visit Europe for the purpose of studying the various processes employed for timber impregnation. The results of von Schrenk's investigations are embodied in an elaborate report, called "Decay of Timber and Methods of Preventing it."¹ According to this report (in which railway sleepers are more especially dealt with), in England, Belgium, and France the creosoting process is the one chiefly used. Although it is the most costly, it is found to pay best in the end in these countries. In Germany and Austria a cheaper process, making use of zinc chloride and tar oil, is now employed, whilst in some other lands the still cheaper zinc chloride process finds favour. In England and France engineers believe that their system gives the best results, and they use as much of the impregnating material as the timber will hold. The extra first cost of the sleepers is amply paid for, as their life is increased by many years. Creosoted pinewood sleepers, in some cases, have lasted twenty-five years or more. Timber wholly impregnated with creosote never rots, but may be worn out by mechanical abrasion. The advantage of using zinc chloride and coal tar in conjunction is supposed to be that the coal tar prevents the leaching out of the zinc chloride. When the latter is used by itself, it disappears from the sleepers in course of time.

About ten years ago a process of wood preservation known as the Hasselman treatment² was developed. Salts injected into wood are soluble in water, and hence liable to leach out. The Hasselman treatment is supposed to cause the injected salts to form an insoluble compound with the wood cell-walls. The timber to be treated is put into a closed cylinder, and a solution of copper sulphate, iron sulphate, aluminium sulphate, and a small amount of kainit is run in. By means of superheated steam the solution is brought to boiling-point. The timber is boiled in the solution for several hours. The advantages of the process lie in the cheapness of the salts employed, the rapidity of the process, the fact that the wood to be treated may be wet or dry, and also that the iron and copper salts penetrate every fibre of the wood and form insoluble compounds with the wood

¹ U.S. Department of Agriculture, Bureau of Plant Industry, Bull. No. 14, 1902.

² H. von Schrenk, *loc. cit.* p. 55.

substance. The first experiments with the process were made by the Bavarian Government, but since that time a number of private companies have used this method of treatment.

Certain newer methods of timber impregnation, such as (1) the senilisation process, (2) the emulsion treatment, (3) the creo-resinate process, and (4) the Ferrell process, were found by von Schrenk in 1902 to be still undergoing trial. In the senilisation process, which is used by a French company, the wood is impregnated with magnesium sulphate by means of an electric current. The emulsion treatment is an attempt to reduce the amount of the tar oil required. One of the methods of doing this is to employ an emulsion of tar oil made by dissolving various quantities of resin in the tar oil and adding a strong solution of soda lye (NaHO). In the resulting emulsion the tar oil is found in the form of exceedingly minute drops, which remain in the emulsified state for several weeks. The emulsion, when pressed into wood, distributes the tar oil evenly throughout its outer layers. The creo-resinate process is like the emulsion treatment, but instead of adding lye the inventor adds formaldehyde. The wood is first subjected to a high degree of heat, to kill any organisms present in it. After creating a vacuum the impregnating solution is run in. The process is supposed to render the wood absolutely sterile at first, and then to give it additional protection outside. The Ferrell process is of American origin. The inventors state that they can inject various salts, such as aluminium sulphate, sodium chloride, calcium chloride, etc., into timber through its ends. They say that the salts penetrate all the fibres thoroughly, and that they can cause the union of two salts in the wood, forming insoluble compounds.¹

¹ For literature upon timber decay and impregnation, *vide* the bibliography to von Schrenk's Report.

THE SOUTH-EASTERN COALFIELD, ITS DISCOVERY AND DEVELOPMENT

By MALCOLM BURR, B.A. (Oxon.), F.G.S., F.L.S.

I. PROPHECY

IN the first Memoir of the Geological Survey (vol. i. p. 214) in 1846, de la Beche remarks: "From the movement of the older rocks many a mass of Coal Measures may be buried beneath the Oolites and Cretaceous Rocks on the east, the remains of a great sheet of the accumulations connecting the districts we have noticed, the Mendip Hills, with those of Central England and Belgium, rolled about and partially denuded prior to the deposit of the New Red Sandstone."

This is the embryo of the idea which was developed by Godwin-Austin in 1858, but not proved till 1890.

It is true that in 1848, in a work entitled *Winning and Working Coal* (p. 19), Dunn commented upon the physical continuity of the chalk, which conceals the Coal Measures of Belgium, with that of Dover, and, in his own words, this "raised the very curious and important question as to whether or not the carboniferous coalfields of Belgium exist under the similar chalk formations in Britain" (quoted by Joseph Lucas, *Trans. Institut. Surv.* vol. ii. Part 12, p. 475, 1908). But there is no relation of cause and effect between the Chalk and the Coal Measures, and Dunn's argument is scarcely more scientific than the old prejudice of an ignorant public, which refused to admit the possibility of coal existing beneath the Chalk.

When de la Beche wrote, the Pas-de-Calais coalfield had not been discovered; but an artesian boring through this formation at Oignies, on the borders of the Nord and the Pas-de-Calais, started in 1841, ending in 1846 at a depth of about 1,312 ft., had struck the Coal Measures at a depth of 495 ft. and passed through several thin seams.

Had Dunn been aware of this momentous discovery, he might have laid greater emphasis on his argument, and he certainly would have referred to it in his attempt to connect the hidden

coalfield of Mons with the South of England. It is to Godwin-Austin that the credit belongs, for he was the first to elaborate the theory and to trace the connection between cause and effect, in that he first enunciated the now generally accepted principle that recent folds in the earth's crust correspond to the deep-seated folds in the basement rocks beneath. In other words, the line of a long-since buried mountain-chain could be detected by physical features at the surface at the present day. In the hand of an experienced geologist this principle is a most valuable instrument, but it requires care and deliberation in its employment.

At the beginning of the Permian Epoch, those infinitely remote days which followed the deposition of the Coal Measures, the latter extended in an unbroken sheet over vast areas of the world. It is probable that a continuous forest stretched from South Wales across Europe into Asia, perhaps even as far as Japan. During the Permian Epoch a great series of earth movements set in, attributed to the contraction of the earth's crust due to the secular cooling of the interior of the planet. As the earth shrank, wrinkles were formed on its surface; these wrinkles ran in two main series, one north and south and the other east and west; the tops of these ridges were exposed to the inclemencies of the weather, and the deposits of Coal Measures were washed away from these summits, down into the valleys where they reinforced the already existing deposits. The tops of these wrinkles or folds are termed anticlines, and the lower parts synclines. It follows that, if we could trace these on the map, we could mark out the coalfields, or coal-basins, which lie in the synclines, and avoid wasting trouble and time by looking for coal on the anticlines.

This is exactly what Godwin-Austin did. He traced a great anticline across the south of England, from Pembroke across the Mendips, past Kingsclere and Maidstone, along the North Downs towards Folkestone, where it makes a bend to the south and is continued in France from Cape Grisnez along the hills of Artois into Europe. This is called the Pembroke-Mendip anticline by Boyd Dawkins, but is more generally known as the Axis of Artois. It is really a buried mountain range, composed of very ancient rocks, older even than the Coal Measures, which repose upon the floor thus provided. These ancient rocks appear at the surface in Pembroke; then

they plunge beneath the newer beds; the Mendip hills are simply a covering of newer rocks laid down over this ridge. The Upper Greensand Inlier at Kingsclere and the Weald Clay Inlier at Maidstone are, to the geological eye, due to its influence, and the high ground of the chalk escarpment, along the edge of the North Downs, betrays its presence: this new ridge of the North Downs is cut open by that geological ditch the English Channel, and it requires little imagination to realise from the cliffs of Folkestone the physical continuity of the chalk Downs of Kent with the white cliffs of Grisnez on the French coast opposite. In France the old rocks are rising near the surface. The hills of Artois are but a thin sheet of chalk over the old Devonian rocks beneath, and these are sometimes even struck by quarrymen near Clarence and Liévin.

Now from what has gone before it is evident that the tracing of this line is a most valuable achievement. It shows us where not to look for coal. But we might reasonably hope to find it at some distance to the north or to the south of the anticline. It should be remembered that the general opinion of geologists at that time was that the Mesozoic rocks extended in complete succession in the south-east of England down to a great depth, and Godwin-Austin's views met with considerable scepticism. Murchison refused to the last to believe in the existence of the old palæozoic rocks at an accessible depth in this district. But before his paper was printed, a boring at Kentish Town struck the old palæozoic floor immediately beneath the Gault, and a little later a black slaty rock, of apparently Primary age, was struck at Harwich, also beneath the Gault.

The Lower Greensand crops out with such regularity all round the London basin that few were prepared for any break in the succession; but all open-minded geologists were bound to accept the evidence of these borings, and Prestwich did not hesitate to set the example. Shortly afterwards another boring, at Meux's brewery in Tottenham Court Road, struck the Devonian rocks at a depth of about 1,000 ft., beneath 64 ft. of Lower Greensand.

II. FAITH

These discoveries aroused so much interest, that there was great speculation as to the depth at which the palæozoic floor would be met with beneath the Weald.

In order to settle this question, the famous Subwealden Boring was promoted by a powerful committee, on the occasion of the visit of the British Association to Brighton in 1872.

To meet the expenses, a subscription list was opened, which eventually included 333 names; the Treasury granted £1,000 (of which £900 was drawn), and various public bodies contributed handsomely.

Among the list of subscribers we find the following noteworthy names:

H. Bessemer, F.R.S., £25; Charles Darwin, F.R.S., £30; Prof. W. Boyd Dawkins, F.R.S., £5; the Duke of Devonshire, £550; H.I.M. The Emperor of the French, £5; Robert Etheridge, F.R.S., £5; Francis Galton, F.R.S., £10; David Forbes, F.R.S., £10; R. A. Godwin-Austin, F.R.S., £45; Sir John Hawkshaw, F.R.S., £150; Prof. E. Hull, F.R.S., £6 2s.; Sir John Lubbock, F.R.S., £50; Sir Charles Lyell, £15; the Duke of Norfolk, £75; Prof. Prestwich, F.R.S., £10; Prof. Ramsay, F.R.S., £5; the Duke of Richmond and Gordon, £10; C. W. Siemens, F.R.S., £30; W. H. Smith, M.P., £40; William Topley, F.R.S., £5; Sir Edward Watkin, M.P., £5; Henry Willett, F.R.S., £350.

It was to the energy and initiative of Henry Willett that the exploration was due, and the committee included the names of all the leading geologists of the day. We may specially note the names of Godwin-Austin himself, and of Boyd Dawkins, who has been associated with Kent Coal until quite recently.

The total amount of money spent was £6,122 7s. 1d., but the results of the boring, from our point of view, were purely negative. The hole attained the depth of 1,905 ft., but the lowest core brought up was doubtfully referred to the Oxford Clay.

An attempt was now made to raise sufficient funds to explore in Kent. But the disappointment of the first exploration dried up the source of supplies, and though Sussex had given such splendid support to the Subwealden Exploration, Kent failed to find the funds, and this aloofness has ever since characterised the attitude of that county to the discovery and development of its great buried wealth.

A sub-committee, consisting of Messrs. Godwin-Austin, Ramsay, Prestwich, and Evans, drew up a report, of which the following noteworthy paragraph is worth quoting: "We should

therefore recommend this trial to be made in the neighbourhood of Dover or the valley of the Stour, a short distance south of Canterbury, where it is probable that the thickness of the Chalk and Gault will not be found to exceed 700 to 1,000 ft., and where the other secondary strata may be expected to be comparatively thin."

This is the first indication of the north-easterly attenuation of the secondary rocks beneath the Gault, insisted upon by Boyd Dawkins in his various reports to the Kent Coal Concessions, which has been proved up to the hilt by various borings at Dover, Ellinge, Ropersole, Waldershare, Fredville, Goodnestone, Barfrestone, and Woodnesborough.

III. WORKS

After this failure the question again lay dormant for many years. Impetus was at length given by Mr. Chamberlain's veto on the Channel Tunnel Scheme. Sir Edward Watkin, persuaded that the prohibition would shortly be removed, and wishing to keep his staff employed, put down the famous boring at Dover in 1886. This was carried on for four years, and workable seams of coal were struck in 1890.

It is to the enterprise of Sir Edward Watkin that the credit is due, and this has been lost sight of amid the wrangles of his engineer and his geologist.

The results of this famous boring are familiar; the section was briefly as follows:

	Thickness
Grey Chalk and Chalk Marl	130 ft.
Glauconitic Marl	8 "
Gault	121 "
Lower Greensand and Wealden	246 "
Oolites and Lias	608 "

The Coal Measures were entered at 1,113 ft. below high-water mark, and penetrated to a depth of 2,200 ft., after passing through eight seams of workable thickness, varying from 2 to 4 ft.

The long tale of subsequent disaster and delay is familiar to everybody. It is scarcely to the credit of our commercial enterprise that eighteen years have been allowed to elapse since the seams were struck, and coal has not yet been raised in marketable quantity. Let us compare for a minute the progress made by our neighbours in the Pas-de-Calais:

- 1843. Coal discovered by chance at Oignies.
- 1847. Two seams discovered by boring at Escarpelle.
- 1851. Output of 4,672 tons at Courrières.
- 1855. Numerous borings proved continuation of the basin from Douai to Fléchinelle, a distance of 65 km., or about 40 miles.

By the end of 1855, nine concessions had been granted, comprising a total area of 36,624 hectares, equal to about 90,000 acres. Coal was placed on the market as early as 1851, and in 1862 the output from this recently discovered coalfield was over a million tons. When the coalfield had been discovered as long as ours has, the annual output had almost attained the figure of a million and a half. A pit begun on April 1, 1851, put out coal in 1855.

The Coal Measures are reached at a depth of about 500 ft. The overburden consists entirely of Cretaceous rocks, but it was of course often necessary to penetrate to a greater depth in order to attain a workable seam.

In England progress has been snail-like: hampered by an ignorant and an obstinate public, the struggles for success have been carried on against overwhelming difficulties, and though Kentish Coal has not yet been placed on the market, the day is now very near at hand when we shall see coal being sold at the pit's mouth in the downs of East Kent, nearly twenty years after the first striking of coal.

Six years were allowed to pass before any steps were taken to profit by the discovery. In 1896 a private syndicate began the pit at Shakespear's Cliff: handicapped by a great thickness of water-bearing Greensand and Hastings Sands, subject to infiltration of marine water through the porous Folkestone Beds, in a site cramped for want of space, where there can never be room to handle a large output, the first attempt at a colliery has eked out a precarious existence for twelve years, and to-day seems as far as ever from attaining its objects.

The authorities were so preoccupied by their works on the foreshore that they neglected to explore inland. Other companies carried out borings at Hothfield, Old Soar, Ottinge, Pluckley, Penshurst, Brabourne, Ropersole, and Ellinge. This first batch of explorations was hampered by want of funds; the boring at Old Soar never passed out of the Weald Clay (467 ft.); Ottinge only showed 367 ft. of Lower Greensand;

Hothfield, after 105 ft. of Lower Greensand, stopped when it had penetrated 513 ft. into the Weald Clay; Pluckley attained a depth of 1,698 ft. without touching the palæozoic rocks; Penshurst met the same fate. But Ellinge and Ropersole proved the Coal Measures, though both died of starvation before attaining any workable seams.

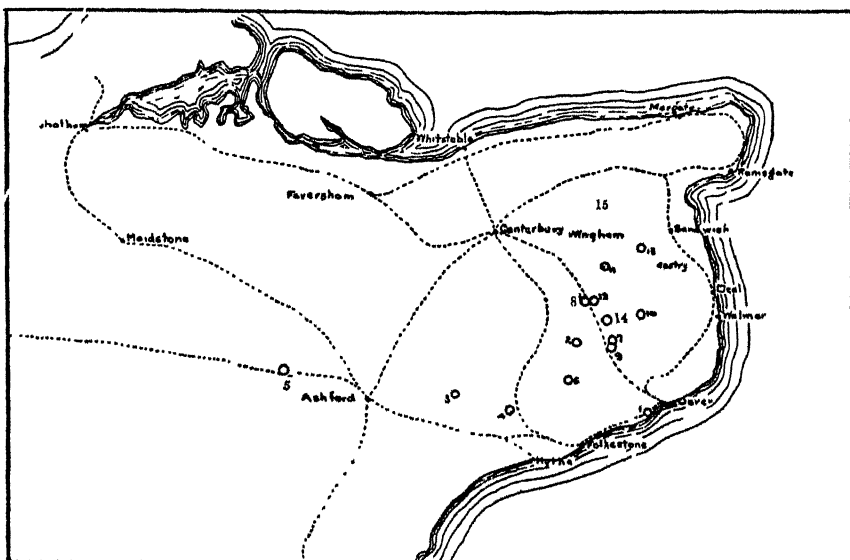


FIG. I.

Sketch Map of East Kent, showing position of Borings and Sinkings

- | | | |
|--------------------------|--------------------------|---------------------------|
| 1. Dover Boring and Pits | 6. Ellinge Boring | 11. Godnestone Boring |
| 2. Ropersole Boring | 7. Waldershare Boring | 12. Snowdown Colliery |
| 3. Brabourne Boring | 8. Fredville Boring | 13. Woodnesborough Boring |
| 4. Ottinge Boring | 9. Guilford Colliery | 14. Barfreston Boring |
| 5. Pluckley Boring | 10. Tilmanstone Colliery | 15. Walnstone Boring. |

The section of these two latter holes is as follows :

Wealden-Furbeck	Ropersole (Boyd Dawkins) ft.	Ellinge ft.
Chalk	834	574
Gault	119	164
Lower Greensand	72	58
Wealden	15	130
Kimmeridgian	10	186
Corallian	157	164
Oxfordian	142	198
Bathonian	164	154
Lias	27	54
Coal Measures at	1,575 below the surface, or about 1,175 ft. below O.D.	1,685 below the surface, or about 1,419 ft. below O.D. ¹

¹ Boyd Dawkins gives 1,000 ft. above O.D. as the altitude of the Ellinge bore, but it was determined by Mr. Stilgoe as 466'1 ft. above O.D.

These three borings, Dover, Ellinge, and Ropersole, remained the only ones which proved the Coal Measures, until the renaissance of Kent Coal in 1905.

The Brabourne boring was very important; by attaining the Devonian Rock at a depth of 1,921 ft., it demonstrated the existence of the anticline at that point, and this limited the south-easterly extension of the coalfield.

The section is as follows :

72 ft. 6 in.	. . .	Gault
230 ft. 6 in.	. . .	Lower Greensand
404 ft.	. . .	Wealden
14 "	. . .	Portlandian
242 "	. . .	Kimmeridgian
305 "	. . .	Corallian
243 "	. . .	Oxfordian
189 "	. . .	Bathonian
74 "	8 in. . .	Lias
97 "	. . .	Trias
		Devonian

IV. THE DARK AGES

With the stoppage of the Ellinge boring in 1901, Kent Coal fell into a period of stagnation, of which the monotony was only relieved by occasional news from the ill-fated pits at Shakespear's Cliff.

V. THE RENAISSANCE

But in 1904 there came into existence a new company named the Kent Coal Concessions, Ltd. After working in silence for some months it started a boring at Waldershare Park, in the Earl of Guilford's estate, at an altitude of about 335 ft. above O.D.

This boring was eventually carried down to a depth of nearly 3,000 ft., and gave the following sections :

Thickness		Depth
813 ft.	Chalk and Marl	
163 "	Gault	976 ft.
70 "	Lower Greensand	1,045 "
42 "	Wealden	1,086 ft. 6 in.
307 ft. 6 in.	Oolites and Lias	1,394 "
	Entered the Coal Measures at 1,394 ft.	

The following is the section of the Coal Measures in the neighbourhood of the seams :

	Thickness	Depth
ay bind	45 ft. 6 in.	1,816 ft. 11 in.
am: struck Sept. 1906	1 " 8 "	1,818 " 7 "
Fireclay	6 " 8 "	1,815 " 3 "
Grey bind	48 " 9 "	1,818 "
Alexandra Seam	3 " 4 "	1,881 " 4 "
Fireclay	5 " 10 "	1,887 " 2 "
Grey bind	16 " 1 "	1,904 " 3 "
King Edward Seam	4 " 6 "	1,908 " 9 "
Fireclay	2 " 3 "	1,911 "
Grey bind	44 " 4 "	1,955 " 4 "
Watkin Seam	1 " 4 "	1,956 " 8 "
Fireclay	5 " 2 "	—
Rockefeller Seam	5 " 2 "	2,372 ft.

Early in the following year the boring at Fredville entered the Coal Measures. This hole is situated alongside the railway, half-way between the stations of Shepherdswell and Adisham. It gives the following section :

Thickness		Depth
800 ft.	Chalk	800 ft.
147 "	Gault	947 "
52 " 4 in.	Lower Greensand	1,000 "
35 "	Wealden	1,035 "
350 " 6 "	Oolites and Lias	1,375 "

entering the Coal Measures at a depth of 1375 ft. below surface, or about 1,111 ft. below O.D. Almost at once seams were struck: the following is the section in the Coal Measures :

Thickness		Depth
21 ft.	Strong bind	1423 ft.
1 " 6 in.	Castor seam	1424 " 6 in
2 "	Fireclay	1426 " 6 "
1 "	Shale	1427 " 6 "
16 " 6 "	Bind	1444 "
1 " 3 "	Shale	1445 " 3 "
1 " 6 "	Pollux seam	1446 " 9 "
6 "	Fireclay	1447 " 3 "
51 " 3 "	Shale and Sandstone	1505 " 6 "
4 " 4 "	Beresford Seam	1510 " 10 "
2 "	Fireclay	1512 " 10 "

The coal from these seams is bright and bituminous. Samples have been analysed by Mr. George R. Hislop, of Paisley, who reports the following results :

Proximate Analysis.	Rockettelli Seam.	Beresford Seam.
Volatile matters	40'23 per cent.	28'31 per cent.
Fixed Carbon	52'66 "	60'67 "
Sulphur	1'9 "	0'76 "
Ash	5'21 "	10'26 "
		<u>100'00 per cent.</u>
Ultimate Analysis.		
Carbon		78'03 per cent.
Hydrogen		5'44 "
Oxygen		4'67 "
Nitrogen		'84 "
Sulphur		'76 "
Ash		<u>10'26 "</u>
		<u>100'00 per cent.</u>

Evaporative power in lb water from and at 212° F. 14'73 13'8

(The high percentage of ash is due to the difficulties in removing dirt from a borehole-sample.)

For comparison we may give the analysis of the 2 ft. 9 in. seam at 1813 ft. from the Dover boring:

Carbon	83'80
Hydrogen	4'65
Oxygen	3'23
Heating power	14'867

The fireclays which form the floor of some of these seams are of good quality. The floor of the Beresford Seam at Fredville appears to be exceptionally fine and adapted to the manufacture of high-class pottery and ornamental ware; so that, apart from their value as refractory materials, they will give rise to a subsidiary industry which will rival the Potteries of the Midlands.

The cores of both these borings have been thoroughly searched for fossil plants by Mr. E. A. Newell Arber, F.G.S., who was able to determine twenty-two species from Waldershare and fifteen from Fredville. Mr. Arber refers both these series to the Transitional Coal Measures, thus correlating them with the Lower Pennant Series of South Wales, with the Potteries Coalfield, with the South Lancashire Coalfield, and with Zeiller's Zone C, which comprises nine-tenths of the Pas-

de-Calais Coalfield, and is the cause of the great wealth of that basin.

Apart from the usual *Calamites*, *Cordaites*, *Stigmaria*, *Sigillaria*, and *Lepidodendron*, Mr. Arber found various species of *Neuropteris* abundant at Waldershare, but less so at Fredville. The chief species are *N. rarinervis* and *N. scheuchzeri*; *Alethopteris lonchitica* occurred, and at Fredville *A. Serli* is numerous.

The whole of these results are discussed by Mr. Arber in a paper shortly to be read before the Geological Society of London.

The geological results of the two borings of Waldershare and Fredville have been discussed by Prof. Boyd Dawkins, F.R.S., in a paper read before the Society of Arts (*Journ. Soc. Arts*, vol. lv. p. 450, 1907), in which the author endeavours to correlate the Waldershare Grit with the Pennant Grit of South Wales, and he determines the Lower Shales Series at Fredville. It is, however, rash to base conclusions on such insufficient evidence, and to assume a former physical continuity between the Grits of East Kent and those of South Wales, more especially as at that time the plant remains had not even been collected.

Unfortunately, the bulk of the Mesozoic cores from Waldershare have been destroyed or rendered useless by the loss or mingling of the labels. This occurred at a time when it was necessary to have police protection at the bore-hole, just before entering the Coal Measures, and in the confusion the Jurassic cores were badly damaged. For three weeks a watch was kept day and night. The guard must have found the Bathonian limestone a rather calcareous bed, though the Oxfordian Clays must have been softer; these big and heavy cores were arranged on the floor of the shed, and were trampled out of recognition. We look to Prof. Boyd Dawkins for the account of this part of the section, for he was Geologist in charge and he alone has the necessary material.

The Neocomian and Jurassic cores of the Fredville boring are fortunately undisturbed: in Boyd Dawkins' paper, above referred to, they are dismissed in the comprehensive term "Oolites," and 10 ft. are attributed to the Lias, but as no description of the lithological character of the ground is there given, an outline of the section may be interesting. When the fossils have all been determined, an authoritative description of the section

will be published. In the meantime, a provisional account is as follows :

<i>Base of the Gault at 947 ft. 8 in.</i>		
Thickness.		Depth.
<i>Lower Greensand (52 ft. 4 in.)</i>		
2 ft. 4 in.	Folkestone Beds	950 ft.
33 "	Sandgate Beds	983 "
	Hythe Beds	absent
17 "	Atherfield Clay	1000 ft.
<i>Wealden Series (35 ft.)</i>		
6 ft. 6 in.	Weald Clay	1006 ft. 6 in.
28 " 6 "	Hastings Beds	1035 "
<i>Oolites (350 ft. 6 in.)</i>		
54 ft.	Corallian	1089 ft.
128 "	Oxfordian	1217 "
43 "	? Kelloway Rock and Cornbrash	1260 "
91 " 6 in.	Bathonian	1351 " 6 in.
24 "	? Lias	1375 " 6 "

The *Folkestone Beds* (947 ft. 8 in. to 950 ft.).

The Zone of *Ammonites mammillatus* is represented by 2 ft. 4 in. of a glauconitic phosphatic conglomerate, with pebbles of Lydite and quartz, some rolled, some angular, up to $\frac{1}{2}$ in. diameter, some being highly polished; there are also irregular phosphatic nodules. There is a fragment of an *Anthoceras* sp. which is not *A. mammillatus*. This bed is undoubtedly identical with the *A. mammillatus* Bed at the outcrop at Folkestone, and of the Dover shafts. The characteristic hard concretions are invariably met with in the borings in East Kent, and are generally referred to as the "hard band," showing that, though remarkably attenuated, this bed is very persistent.

The *Sandgate Beds* (950 to 983 ft.).

Immediately beneath we find 5 ft. of a glauconitic muddy sand, a washing from the top of the Sandgate Beds. The ground becoming firmer, we get a glauconitic mixture of clay and sand, resembling the same ground in the Dover pits, down to 967 ft., where it becomes more glauconitic and contains more pebbles. At 968 to 969 ft. it has passed into a greensand with a little clay. From 969 to 983 ft. we only have a sediment, which probably represents the *Pholas* Band of the Dover section.

Thus we have a total thickness of 35 ft. that can be safely attributed to the Sandgate Beds. In the other borings these beds are persistent, but more attenuated and very argillaceous.

The *Atherfield Clay* (983 to 1,000 ft.).

From 983 to 987 ft. we find a sandy facies of this clay, containing nodules slightly phosphatic; it is also somewhat micaceous; from 988 to 993 ft. it is a fine micaceous silt. At 993 ft. we get the true brown Atherfield Clay, down to 1,000 ft. In the arenaceous character of its upper part it resembles the clay in the Isle of Wight rather than at Dover. It is a remarkably persistent bed, and we find it thickening somewhat from Fredville eastwards and northwards. It is this thickening of the Atherfield Clay at the expense of the Sandgate and Folkestone Beds that is the most striking feature of the subterranean extension of the Lower Greensand in East Kent. From the point of view of the mining engineer this is of course highly satisfactory.

The total disappearance of the Hythe Beds is a very remarkable feature, as they are so strongly developed at their outcrop not many miles distant.

The *Weald Clay* (1,000 to 1,006 ft. 6 in.).

The Weald Clay is sharply distinguished from the Atherfield Clay in a core raised from 1,000 ft., in which the upper portion is light brown in colour and solid, while the lower portion is blue-grey, splits easily along the bedding-planes, and is covered with *Cypridea valdensis*. Some of the bedding-planes are literally crowded with *Cypridea*, and contain numerous vertebræ and scales of *Lepidotus* and other fishes. At 1,005 ft. there is a band of strong, hard clay-stone. The thickness of the Weald Clay in this section is only 6 ft. 6 in.

The *Hastings Beds* (1,006 ft. 6 in. to 1,035 ft.).

These beds begin at 1,006 ft. 6 in., and are 28 ft. 6 in. thick—a remarkable and satisfactory attenuation, for they are apt to be troublesome sinking ground. The upper portion is a pale greenish-grey silty clay, with a few traces of plant remains: below 1,009 ft. it becomes more argillaceous, resembling the "indiarubber clays or muds" met with at 490 ft. in the Dover pits; at 1,012 ft. it becomes more silty, and we find the indiarubber clays interlaminated with the silt; at 1,019 ft. there is a nodule containing *Cypridea*, casts of shells, and fish remains; at 1,025 ft. there is a vertical slicken-sided plane, evidently due to interstitial movement within the mass; at 1,026 ft. it is exceedingly pale buff, almost creamy, in colour; from 1,028 ft. it becomes more argillaceous and *Cypridea* and fish remains more abundant;

at 1,032 ft. it again becomes more silty. At the extreme base we find about 1 ft. represented by dark sandy washings, evidently identical with the loose gravelly base of series which occurred at Dover, resting upon the Kimmeridge Clay.

? *Corallian* (1,035 to 1,089 ft.).

Beneath the Hastings Series we should naturally expect to find the Kimmeridgian, but from this point the Fredville section differs from that of Dover. Instead of a clay, we find the Hastings Beds reposing upon a substantial mass of limestone, 54 ft. thick, which is provisionally referred to the Corallian.

This bed of limestone passes from a coarse pisolite through an oolitic structure, to a dense compact fine-grained calcareous rock, but towards the base it resumes the oolitic texture. The top part is decomposed to a depth of about a foot, forming an earthy pisolite, with indurated masses of calcareous rubble, of a dark grey-brown colour, but lighter in places: from 1,036 ft. it is exceedingly hard; at 1,043 ft. the grain becomes finer, oolitic rather than pisolitic; a small belemnite occurred at 1,046 ft., at which point it is somewhat argillaceous; at 1,048 ft. it is full of shells, more or less crushed, and passes into a very hard limestone, with a fairly even fracture, scarcely oolitic in texture and blue in colour; at 1,050 ft. 6 in. it becomes rather marly, and even finer in texture; about 1,059 ft. it is pyritous, with many fragments of *Gryphæa* and other shells. These become more and more numerous as it passes (about 1,061 ft.) into a hard marlstone; at 1,062 ft. we find a part of a large *Ammonite*, many *Pecten*, numerous pale grey pyritous markings. About 1,063 ft. it becomes once more rubbly, and by 1,065 ft. it is coarsely oolitic and full of ferruginous grains like shot; at 1,066 ft. it has become a regular millet-seed iron-ore, with a big *Gryphæa*. By 1,077 ft. the iron-shot are polished and bright coffee-coloured, just like the iron-stone at Dover; it is full of shell remains, with *Astarte* and belemnites, mostly casts, in a brown marlstone matrix. At 1,083 ft. the shot have become rarer, and at 1,088 ft. they have disappeared, but the ground is pyritous and full of fossils. At 1,089 ft. we have the base of this bed of limestone rock and pass into the clays. There is no Coral Rag.

Oxfordian (1,089—1,217 ft.).

This is a massive bed, 128 ft. thick, of a pale grey marly clay, with a conchoidal fracture and abundant casts of fossils; *Serpula* (?), *Pinna*, *Rhynchonella*, *Thracia*, *Gryphæa*, *Trigonia*,

Astarte, *Ammonites* and *Crustacea* (?) are varyingly abundant; about 1,193 ft. it becomes somewhat calcareous and harder, but near the base it is decidedly sandy.

This massive clay bed is a very constant feature in all the borings.

? *Kelloway Rock and Cornbrash* (1217—1260 ft.).

Between the Oxford Clay and the Bathonian limestone we find fifty-three feet of doubtful age; the upper part is provisionally referred to the Kelloway rocks passing into Cornbrash, but this determination remains to be checked by the identification of the fossils.

The first foot is a sandstone rendered muddy by the admixture of Oxfordian Clay. It is crowded with *Gryphæa* and casts of *Pecten*; at 1,218 ft. it passes into a sandy loam-rock, full of fossils, including *Arcula* and *Terebratula*, very like Cornbrash. This passes into a hard, shelly, calcareous grit, containing a little iron-shot; by 1,220 ft. it is a very dark rich green, very hard grit, full of shining grains, and very compact; this is of a peculiar and altogether distinctive appearance. The iron-shot becomes more and more numerous; from 1,224 to 1,230 ft. they are all brown, and not coffee-coloured, as in the Corallian above and in the Corallian at Dover; from 1,230 ft. 6 in. to 1,233 ft. 6 in. it is a brilliant dark green glauconitic marlstone rock, with many white shells, chiefly *Avicula*. Then there is a 6-in. band of pale grey marlstone, then again as before, only slightly brighter green in colour, down to 1,236 ft. 6 in.; from here down to 1,245 ft. we find a washing of rather coarse angular fragments, but at 1,246 ft. it is the same curious green rock, though not so brilliant, still with *Avicula*; at 1,247 ft. it is dappled with black clay, coarsely Oolitic, with *Rhynchonella*. From 1,257 ft. it is a hard green marlstone crowded with coffee-coloured polished iron-shot, very like the Corallian iron-ore of the Dover pits, with *Gryphæa*, pyritous wood, belemnite of the *B. oweni* type, and *Modiola*; between 1,259 and 1,260 ft. it changes to a soft muddy dark greyish sandstone.

It will be interesting to determine the exact age of this curious green rock, which has not before been noticed from any Kentish boring.

Bathonian (1,260 ft.—1351 ft. 6 in.).

The Bathonian is represented by a massive oolitic limestone: at the top it is hard, dense, and white in colour, a trifle muddy

in places and sometimes gritty, with softer bands, fairly rich in fossils. At 1,287 to 1,289 ft. there is a thin band of dark purplish slicken-sided clay; at 1,290 ft. a poorly oolitic marl, with lignite, shell remains, and a big coral. At 1,293 ft. is a highly fossiliferous band, with crystalline calcite and numerous casts of shells and corals. The texture is generally medium oolitic, in places barren, in others richly fossiliferous. From 1,345 ft. to 1,351 ft. 6 in. we find only a washed sandy clay, which probably marks the base of the Bathonian.

? *Lias* (1,351 ft. 6 in. to 1,375 ft. 6 in.).

It is with much doubt that these twenty-four feet of marlstone are provisionally referred to the Lias. At the top we have a fine grey calcareous sandstone, rich in shell remains, passing into an arenaceous limestone, with belemnites, then again to a compact blue limestone full of *Rhynchonella*: with a marl parting at 1,367 ft. 6 in.; at 1,369 ft. it is a fine sandy dark grey marlstone with *Rhynchonella* and casts of bivalves, which becomes tougher, more compact and more calcareous. At 1,371 ft. 4 in. there is a dark grey laminated shaly clay, with a few inches of clayey wash; finally a laminated silty clay, with a small belemnite, recalling *B. acutus*, resting directly on the Coal Measures, with no distinct basement bed.

There are several curious features about this section: we may specially note the disappearance of the Hythe Beds and of the Kimmeridge Clay, the characteristic green rock, and the occurrence of the millet-seed ironstone in the Corallian and Cornbrash (?). At Dover it occurred in the Corallian.

A detailed account of the Ropersole section has not been published, but it is possible to obtain a fairly clear idea of the structure of the mesozoic rocks at the boring by comparing the journal with the section at Fredville. On this somewhat unsatisfactory basis, we can provisionally establish the following section: it should be noted that the type of Fredville is represented rather than that of Dover, in the disappearance of the Kimmeridge clay as well as of the Hythe Bed, and the presence of the green (?) Cornbrash.

Thickness.	Chalk (834 ft.).	Depth.
350 ft.	Chalk with many flints (Upper Chalk).	
160 "	Chalk with few flints (probably Middle Chalk)	
	Zone of <i>Terebratulina gracilis</i> , part of <i>Rhynchonella</i>	
	<i>cuvieri</i>	

Thickness.		Depth.
40 ft.	Nodular Chalk (Grit Bed)	
268 "	Grey Chalk (Cenomanian, with some of the zone of <i>Rhynchonella curvieri</i> included)	
16 "	Chloritic Marl (the sandy base of the Cenomanian, but it is not certain if the true Glauconitic Marl is present)	834 ft.
119 "	<i>Gault.</i>	953 "
	Lower Greensand (72 ft.).	
2 " 4 in.	Green Sandstone. (Folkestone Beds)	955 " 4 in.
48 " 8 "	Clay and sand ; blue and black sandy clay (Sandgate Beds)	1004 "
21 "	Brown Clay (Atherfield Clay)	1025 "
	<i>Wealden Series</i> (65 ft.).	
30 "	Black shale and dark grey marl (Weald Clay)	1055 "
35 "	Silty sand and sandy clay (Hastings Sand)	1090 "
	<i>Oolites.</i>	
156 "	Nodular and compact limestone (Corallian)	1246 "
114 "	Argillaceous limestone (Oxfordian)	1360 "
77 "	Sandy clay and green sandstone rock with many fossils (? Kelloway Rock and Cornbrash)	1437 "
128 "	Hard oolitic limestone with shells and marly partings (Bathonian)	1565 "
15 ft. 9 in.	(? Lias). Marly shale and marly limestone	1580 " 9 in.
	Entered Coal Measures at 1580 ft. 9 in. below the surface, or 1180 ft 9 in. below O.D.	

The results of the two borings may be tabulated thus :

	Ropersole.	Fredville.
Chalk	834 ft.	800 ft.
Gault	119 "	147 " 8 in.
Folkestone Beds	2 " 4 in.	2 " 4 "
Sandgate Beds	48 " 8 "	33 "
Atherfield Clay	21 "	17 "
Total Lower Greensand	72 "	52 " 4 "
Weald Clay	30 "	6 " 6 "
Hastings Beds	35 "	28 " 6 "
Total Wealden Series	65 "	35 "
Corallian	156 "	54 "
Oxfordian	114 "	128 "
? Kelloway Rock and Cornbrash	77 "	43 "
Bathonian	128 "	91 " 6 in.
? Lias	15 " 9 in.	24 "
Total Oolites with Lias	490 "	340 " 6 in.
Coal Measures (at below O.D.)	1180 " 9 in.	1115 "

The ironstone, which, according to Boyd Dawkins, is physically identical with the iron-ore worked at Westbury, in

Wiltshire, was between 12 and 20 ft. thick in the Dover section. Samples were assayed, and the following results are interesting.

Mr. R. H. Harland in forwarding the analysis writes :

"With this I enclose results of analyses of samples of iron ore, together with analysis of specimen, obtained by washing away the clayey matter from the lowest four feet of material, taken at a depth of 604 ft. 4 in.

"The percentage of clean iron ore obtained was 57½ per cent., and a further 6 per cent., containing some clayey matter, gave 33·49 per cent. of metallic iron, the two together making a total of 74 per cent. of iron recovered in the washed material ; or, taking the clear iron grains, only 70 per cent."

Analyses of Samples of Dover Iron Ore, taken at the Works. By Edward Riley, F.I.C., F.C.S., Metallurgist, Analytical and Consulting Chemist.

	Upper 6 feet.	Middle 2 feet.	Lower 4 feet.
Silica	15·10	11·00	12·00
Peroxide of Iron	38·54	55·24	47·83
Protoxide of Iron	6·56	3·59	8·39
Alumina	5·71	5·75	4·88
Oxide of Manganese	30	30	20
Lime	9·94	5·23	4·42
Magnesia	1·55	1·14	1·76
Phosphoric Acid	·97 (Phosphorus ·42)	1·37 (P. ·59)	1·03 (P. ·45)
Arsenic Acid	Nil	Nil	Nil
Sulphur	·084	·063	·16
Oxide of Copper	Nil	Nil	Nil
Carbonic Acid	11·43	4·10	8·48
Combined Water	7·22	9·90	8·38
Moisture	2·08	2·18	1·77
	99·484	99·863	99·30
Metallic Iron, per cent. . . .	32·08	41·46	40·01
Loss on Calcination, per cent.	20·00	15·78	17·70
Metallic Iron, per cent. on Calcined Ore	40·10	49·23	48·61

The same iron ore occurred at Brabourne, Ropersole, Waldershare and the latter borings ; the degree of concentration varies considerably : at Waldershare, a piece of core was assayed, and returned a very poor yield, but the Oolites are almost all heavily impregnated with iron in this hole.

But the chief point of interest in connection with the Mesozoic rocks is that these borings all proved the thinning-out of

the beds between the Gault and the Coal Measures. This is illustrated by the section (Fig. 2) and the following table:

	Brabourne.	Ellinge	Dover.	Ropersole	Waldershare	Fredville
Lower Greensand	230	59	128	72	70	53
Oolites . . .	1407	821	686	528	352	339
Lias	75	54	38	27	5(?)	10(?)

VI. THE UPPER CRETACEOUS ROCKS.

In order to appreciate the significance of the circumstances affecting sinking progress in East Kent, it is necessary to con-

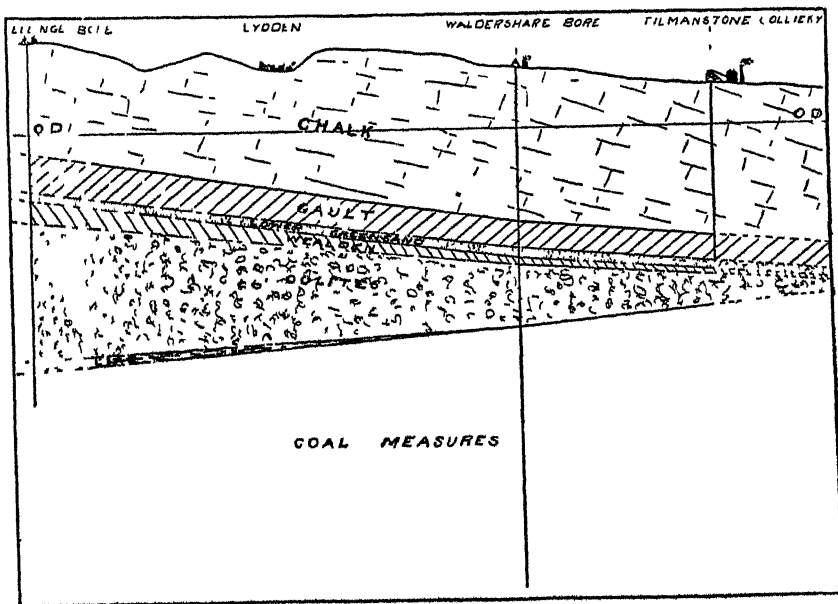


FIG. 2.

Rough Section from Ellinge Bore to Waldershare Bore, produced to Tilmanstone Pit.

Scales Horizontal, 1 in. to 1 mile; Vertical, 100.

sider for a moment the nature of the chalk from the point of view of a mining engineer.

Never before in England have pits been sunk through this formation; consequently the Chalk has been studied only by stratigraphers, palæontologists, and civil engineers interested in water supply. As this formation has a thickness of nearly 800 ft. in the area under discussion, it is evident that it is worth while uniting these different aspects, which is now done for the first time.

The Chalk has long been divided into three members—the Upper Chalk (with many flints), the Middle Chalk (with few flints), and the Lower Chalk (with no flints). As a rough-and-ready arrangement that is easy to remember, this system has been universally accepted.

But within the last few decades it was realised that a more scientific and less empirical arrangement was desirable, more especially as in Devonshire the base of the Middle Chalk is crowded with flints, and the Upper Chalk of Thanet is almost flintless.

D'Orbigny showed that these three divisions correspond approximately with the changes in the Fauna, and that each division is characterised by a special assemblage of fossils. He gave them respectively the names Senonian, Turonian, and Cenomanian.

A little later Charles Barrois, after a meteor-like traverse of the Chalk of Great Britain and Ireland, established the foundations of the Zonal System, which has since been perfected by Dr. Arthur Rowe. The Chalk is divided into a number of zones, of varying thickness, each zone being characterised by a certain set of associated fossils. Each zone is named after some one species, which is also characteristic of the zone by being confined to it, or by being more abundant there than elsewhere.

These zones are as follows :

SENONIAN (Upper Chalk).	
Zone of :	
<i>Ostrea lunata</i>	} absent in Kent.
<i>Belemnitella mucronata</i>	
<i>Actinocamax quadratus</i>	
<i>Marsupites testudinarius</i>	142 ft. thick.
<i>Micraster cor-anguinum</i>	250 "
" <i>cor-testudinarium</i>	56 "
<i>Holaster planus</i>	35½ "
TURONIAN (Middle Chalk).	
<i>Terebratulina gracilis</i>	161 "
<i>Rhynchonella cuvieri</i>	70 "
CENOMANIAN (Lower Chalk).	
<i>Holaster subglobosus</i>	146 "
<i>Ammonites varians</i>	54 "

We will now briefly consider the characteristics of each zone separately.

Zone of Marsupites testudinarius.

This zone, the highest in Kent, is formed of a pure, dazzling white chalk, in which beds of flints are rare: it is divided into prismatic blocks by numerous and well-defined perpendicular joints, and it is this frequent parallel jointing which gives the vertical face to the cliffs at Margate.

A thin flint bed known as the "Bedwell line" divides it into two sub-zones or bands: *Marsupites testudinarius* occurs commonly above, and *Uintacrinus* below it. The gigantic Ammonite, *Amn. leptophyllus*, is well known in the cliffs between Birchington and Kingsgate. The *Uintacrinus*-band differs from the upper sub-zone in the replacement of the former crinoid by the latter: a little belemnite, *Actinocamax verus*, occurs commonly in the lower band, replacing *A. granulatus* which occurs in the upper. The *Uintacrinus*-band is 62 ft. thick in Kent.

It is only met with in one of the three collieries, namely at Tilmanstone, where the top 30 ft. of chalk is in this zone.

Zone of Micraster cor-anguinum.

This zone is about 250 ft. thick on the coast of Kent. It resembles the previous zone in its brilliant white colour, but differs in the abundance of the beds of flint which occur with remarkable regularity. The highly flinty chalk at the top of the cliff under Dover Castle is in this zone, and the projecting jagged fangs of the nodular flint beds are very striking.

Beds of tabular flints sometimes occur: these are continuous sheets of flints, and afford convenient measuring-points when it can be shown that they are persistent; but there is no doubt that they sometimes pass into nodular flint, and *vice versa*. One of the most important is the "3-inch band" of Whittaker, which can be traced from Kingsgate to St. Margaret's. It is a prominent feature in the cliff, and is useful as a guide to the junction with the overlying zone, which is exactly 21 ft. above it.

The fauna is relatively poor, especially near the base. *Micraster cor-anguinum* occurs throughout the zone, but also extends into the zone above; a large urchin named *Echinocorys sculatus*, Breyn, is common enough, and has always a rounded outline: in the zone above, this urchin is distinctly pyramidal.

About 18 ft. above the base of the zone there is a bed of tabular flint, which is familiar to visitors to St. Margaret's Bay. This is known as the *Micraster cor-anguinum* tabular, and is found in the pits inland.

Zone of Micraster cor-testudinarium.

This zone presents a marked contrast to the one above. Flints are numerous, but scattered: it is 56 ft. thick in Kent, the chalk is distinctly harder, discoloured with yellow remains of sponges, and weathers out into hard irregular nodules.

The fossils are abundant, this affording a great contrast to the zone above. The most characteristic kinds are a special form of *Micraster cor-testudinarium* and of *Micraster precursor*, *Holaster placenta* (a large, depressed, thin-shelled urchin), a gibbous variety of *Echinocorys scutatus* and *Serpula ilium*, a little convoluted worm tube.

Zone of Holaster planus.

This zone, only 35½ ft. thick, is perhaps the most interesting of the series. The chalk is hard, sometimes intensely so. It is greyish with marl staining; flints are abundant but scattered. In its upper portion there is a narrow band of intensely hard nodules of silicious chalk embedded in a soft marly matrix; this weathers out inland into a breccia by the removal of the marl. In places the hard band is an unbroken sheet of nodular chalk, so hard that it can scarcely be broken with the hammer, under which it rings like granite.

About the middle of the zone there is a band, 9 ft. thick, of hard chalk with green grains and phosphatic nodules, and a peculiar fauna, for this is the only bed above the Cenomanian in which Gasteropods and Cephalopods occur commonly; of the former *Turbo gemmatus*, *Pleurotomaria perspectica*, and *Trochus schlüteri* occur as casts; and of the latter *Heteroceras reussianum* and *Scaphites geinitzi* are the most characteristic.

This band is the palæontological representative of the Chalk Rock of the Midlands. The indurated silicious bands mentioned above are its physical representatives. These are so hard that they form a feature which can be detected at its outcrop, and so is most useful for mapping.

The junction between this zone and the underlying chalk is marked by a prominent band of marl situated between two

pairs of strong lines of nodular flints. This marl band is the base of the Upper and summit of the Middle Chalk.

Fossils characteristic of the whole zone are a special form of *Micraster cor-testudinarium* and *Micraster præcursor*, *Holaster planus*, the gibbous variety of *Echinocorys scutatus*, *Micraster leskei*, and *M. cor-bovis*.

Zone of Terebratulina gracilis.

The chalk of this zone is massive and firm, with few open joints, and grey with admixture of marl. It is consequently relatively impermeable to the percolation of water, which finds a line of least resistance in the marl bands which occur at regular intervals: these marl bands form false spring planes, and in the sinkings water is not met with in the chalk itself in this zone, but small feeders are generally thrown out by the marl bands, which occur regularly at known intervals. Flints have become rare.

This zone is 161 ft. thick. Fossils are not numerous, but *Terebratulina gracilis* itself, like a pretty little scallop, is common, especially in the marl seams. The primitive *Micrasters*, *Micraster leskei* and *Micraster cor-bovis*, occur near the top of the zone, but are rare, and fragments of a big coarse-ribbed, convex bivalve called *Inoceramus lamarcki* Parkinson (= *brogniarti* Mant.) are common.

Zone of Rhynchonella cuvieri.

The passage between these two zones is taken on the coast at the lowest line of flints, but this does not hold good inland. The real passage is a zoological one, and is marked by the replacement of *Inoceramus lamarcki* by the elongate, depressed, finely striated, mussel-like *Inoceramus mytiloides* (= *labiatus* Schl.); *Rhynchonella cuvieri*, a little convex ribbed cockle, which first appears at the base of the Upper Chalk, now becomes really common, but other fossils are not numerous.

The zone is 70 ft. thick: the upper half is scarcely distinguishable from the zone above in physical characters. The marl is present not so much in clear, well-marked bands as in ill-defined and very numerous streaks and veins. But about the middle, bands of hard nodules appear, and the bottom 32 ft. of the zone consists of an indurated rough nodular chalk known as the Grit Bed, which corresponds to the Melbourne

Rock of the Midlands. Its roughness is due to the presence of innumerable comminuted fragments of *Inoceramus mytiloides*.

Zone of Holaster subglobosus.

The Grit Bed ends abruptly, its under-surface being flat and smooth, so that there is a sharp line of demarcation between it and the underlying bed. This is the junction of the Turonian and Cenomanian, of the Middle and the Lower Chalk.

The top of this consists of a smooth, dark, greenish-blue flaky marl, a striking contrast to the overlying Grit Bed. This bed of marl is about 4 ft. thick, and is characterised by the frequent occurrence of a large belemnite known as *Actinocamax plenus*. This species is common in this band, which is consequently known as the Belemnite Marl, and dies out in the firmer chalk beneath. The Belemnite Marl is very persistent, and can be followed, throughout the Chalk area even into Yorkshire, where it is present as a narrow black carbonaceous seam.

Beneath it we have a considerable thickness of massive, dense, compact, yellowish marly chalk, the true chalk of *Holaster subglobosus*. This is 142 ft. thick, and is good ground to sink through, as it is firm without being too tough, and quite dry. Fossils are rare, but a few sharks' teeth occur, and occasional specimens of the name-fossil.

Zone of Ammonites varians.

This zone has several subdivisions. At the top of the zone is the Cast Bed, a band only about 2 ft. thick rich with fossil remains. The forms to which we have grown accustomed in the Chalk now disappear, for though we find the familiar genera the species are different, and the whole assemblage more resembles the fauna of the Gault.

Beneath the Cast Bed we have a dark blue-grey, impervious, chalk, rich in marl, generally soft, but with numerous very hard bands. Towards the base there are a series of thin stony beds full of sponge remains: this is sometimes called the sub-zone of *Plocoseyphia labrosa*, and is about 10 ft. thick. Beneath these hard reefs there is a remarkable and very distinctive belt of very dark green arenaceous marl. This is the Glauconitic Marl, formerly incorrectly termed the Chloritic Marl, and confused with the Upper Greensand. It is in reality

only a purely local feature, an arenaceous facies of the basement bed of the Chalk, known as the sub-zone of *Stauronema carteri*, a fine sponge which is not rare. Though nearly 16 ft. thick at its outcrop at Folkestone, this bed is not persistent inland. It is scarcely represented in the Tilmanstone pit, but is 4 ft. thick at the Woodnesboro' boring.

The total thickness of the zone of *Ammonites varians* is 58 ft.

The Gault.

Where the Glauconitic Marl is poorly represented, the passage from the Chalk to the Gault is gradual. The base of the Chalk is richer in lime than in marl: the top of the Gault is richer in marl than in lime.

The Upper Gault, or zone of *Ammonites rostratus*, thickens inland, for while the total thickness of the Gault at Folkestone barely exceeds 100 ft., it approaches nearer to 200 ft. as we proceed inland towards the north-east. The following are the figures for the various borings:

Dover	121 ft.
Ellinge	164 "
Ropersole	119 "
Waldershare	163 "
Fredville	147 "
Goodnestone	181 "
Barfreestone (upper limit doubtful: probably over 150 ft. thick).	
Tilmanstone Pit	129 "
Woodnesboro'	150 "

In this connection it is worth noting that borings at Chatham and at Frindsbury, north of Maidstone, proved a thickness of 192 and 193 ft. respectively, and at Trottescliff the bottom was not reached after 183 ft. had been passed through (cp. Jukes-Brown, *Cret. Rocks Brit.*, i. p. 86, *Mem. Geol. Surv.* 1900, and Whittaker, *Q. J. G. S.*, xlii. p. 38, 1886). It will be seen that these thicknesses are by no means exceptional when we note that 181 ft. of Gault was proved at Goodnestone. This thickening to the north is balanced by an attenuation to the south, for the 112 ft. at Dover, and 100 ft. (approximately) at Folkestone is reduced to only 42 ft. at Wissant in the Boulonnais. It seems to be the Upper Gault which suffers; the Lower Gault, that is the zone of *Ammonites laevis* and *Ammonites interruptus*, the part beneath the bed with crowds of crushed *Inoceramus sulcatus*, remaining

fairly constant. In the Tilmanstone pit these two lower zones combined have a thickness of about 40 ft. : at Folkestone they are 35 ft. thick.

It is exceptional, rather than the rule, that a species of fossil should have a sharply defined and restricted distribution. As examples we may quote *Untacrinus*, a free-swimming crinoid, whose unmistakable, deeply-grooved, irregularly pentagonal plates are very abundant in the basal portion of the zone of Marsupites. The belt in which this species occurs is only 62 ft. thick in East Kent, and it is absolutely restricted to this belt : consequently the discovery of one of these plates in a quarry or a pit is certain proof of the presence of that particular horizon.

Another striking instance is afforded by *Actinocamax plenus*. This is a belemnite which occurs commonly in a thin bed of marl which caps the Cenomanian or Lower Chalk. The discovery of this belemnite and its marl seam, only a few feet thick, is an absolutely undeniable proof of the junction of the Middle and Lower Chalk.

Further very valuable evidence is afforded by the genus *Micraster*, a kind of sea-urchin which is abundantly represented in the lower zones of the Senonian or Upper Chalk. Rowe has shown that in the forms from the lower horizons the ornamentation and sculpture of the test are simple, but that it becomes more and more ornate as we trace the evolution upwards. He has worked out in minute details the evolution of the test in an unbroken succession of gradations with such accuracy that it is possible to determine the horizon by the examination of the *Micrasters* alone, and this fact has been of very great value in delimiting the zones passed through in the sinkings. Two species of this genus, namely *Micraster cor-testudinarium* and *Micraster cor-anguinum*, are chosen as indices, or species which give their names to zones : these species are not rigidly confined to the zones in question, but are specially abundant in them.

For zonal work, whether in the field or in the pits, it is not usually enough to find single specimens, but all available material should be searched in order to present a collective idea of the fauna as a whole.

In the sinking of the shafts at the three collieries, the fossils

are collected with care, and information is thus obtained which is as valuable to the engineers in their practical work as it is to the pure geologist.

VII. THE NEW COLLIERIES

So great was the faith of moving spirits in these later explorations, that two trial shafts were started even before the Coal Measures were struck at the Waldershare boring. These two exploratory pits had each a diameter of 7 ft. One was situated in Singledge Lane, about a quarter of a mile south of the Waldershare boring and about a mile north-east of Lydden; the second is in the parish of Eythorne, on the Sandwich road, near the village of Tilmanstone. The former has since developed into the Guilford Colliery, the second is the well-known Tilmanstone Colliery.

Guilford Colliery had attained a depth of nearly 300 ft. when it entered the wet chalk. During the delay caused by the preparation of the pumping plant, coal was struck at the boring, and consequently the programme was enlarged. Instead of widening the 7-ft. pit, it was decided to convert it into a water-pit for the boiler supply, and to commence a pair of full-sized 18-ft. shafts for a real colliery. No. 2 was marked out, sunk the first 40 ft., the pillars erected, and then left to await its future completion. No. 3, the Frances Shaft, was carried down to the water-level, and then operations were suspended for a few months owing to the attitude of the highway authorities, as the bad weather and the works involved considerable damages to the roads, and the consequent expenses were very heavy. In the summer of 1907 operations were resumed, and several months were spent in providing a sinking-plant suitable to the more ambitious programme. The winding-engine was ordered in January 1907: it was not under steam until twelve months later. This illustrates the difficulties which have to be contended with in this kind of work, and the long and tiresome delays which are so irritating to staff and shareholder alike.

When progress was resumed in the pit with the powerful equipment of engines and boilers, it was found that the total quantity of water coming from the combined feeders in the zone of *Holaster planus* and in the marl bands of *Terebratulina gracilis* was easily mastered by means of a water-barrel alone; this pit has the distinction of being the first to pass through

the water-level in the chalk down to the impermeable ground beneath without ever having recourse to pumps. This is an ample vindication of those who maintain that the passage of the water-level in the chalk presents, as a rule, no serious difficulty, and the experience has been since repeated at both the other collieries.

The pit has since been carried to a depth of about 700 ft., well down into the zone of *Ammonites varians*, and has shown all the prominent features that are recorded on the coast section. The thickness of the Middle Chalk is 239 ft., compared with 231 ft. as measured on the coast. The slight apparent discrepancy may be accounted for by the fact that the coast measurements are taken perpendicular to the dip, while the pit is, of course, truly vertical.

The trial pit at Tilmanstone, about a mile and a half to the north of the Waldershare boring, was commenced in August 1906 with a diameter of 7 ft. It is situated on the outcrop of the *Uritacrinus*-band, about 30 ft. above its base. When the depth of 270 ft. was attained, the discovery of the thick seams at Waldershare removed the speculative element, the pit was widened to 14 ft., and operations thus recommenced in October. Water was met at a depth of about 160 ft., but the passage of the water-bearing chalk was accomplished without difficulty - the total feed scarcely exceeding 5,000 gallons per hour: once more a convincing reply to those who fear sinking through chalk.

The pit was rapidly sunk down to the base of the Gault, and finally the Lower Greensands were struck at a depth of a little over 900 ft. These of course are water-bearing;¹ but the conditions being novel, it was impossible to make all preparations in advance. The consequent delay should have been about two months; the manufacturers dragged this out into six months, and during this long wait no yardage was accomplished in the pit, though material progress was effected in numerous ways that were indispensable to the works. Long headings were driven in the hard chalk in the Grit Bed, the indurated base of the zone of *Rhynchonella cuvieri*, and in the waterproof chalk of *Holaster subglobosus* immediately beneath.

At length, by midsummer, the pumping installation was

¹ The quantity of water in the Greensand in this pit has since been shown to be inconsiderable.

almost complete; and it is confidently hoped that this pit, already so deep and so amply equipped, will rapidly be carried down to the Coal Measures, which are expected at about 1,200 ft.

In November 1907 the first sod was dug of the second shaft, the Gabrielle Pit, which was rapidly sunk to a depth of 400 ft. into the dry zone of *Terebratulina gracilis*, through the most dangerous part of the wet chalk, while the ponderous main sinking equipment was being installed. It has since been carried down to a depth of almost 600 ft. into the Grit Bed.

At the close of 1906 the Fredville boring spoke out; and, so soon as the fine Beresford Seam was proved, steps were taken to open out a third colliery in the immediate neighbourhood of the boring. This is the Snowdown Colliery, which, with its tall, slim ferro-concrete chimney, is a familiar sight to the travellers from Canterbury to Dover, as the line passes the works, and is connected thereto by a siding.

Two pits, each with a diameter of 18 ft., were started in the following spring; but these were doomed to try the patience of the explorers, for at a depth of 275 ft. in the zone of *Micraster cor-testudinarium* a strong feeder was struck which drowned out the pit, unfortunately with fatal results. By an evil misfortune the shaft had chanced to meet with one of those open fissures carrying streams which occasionally occur in the chalk or other limestone formations. This fissure, or pocket, was evidently connected with the surface; for instead of being clean and clear, like the other chalk water, this feeder was discoloured by dirt, which, upon examination, appears to be identical in composition with the superficial drift which locally caps the chalk, and no less than 18 ft. of this mud was washed into the bottom of the pit.

The provision and installation of the necessary pumps are a tedious business, as all mining engineers know to their cost; and, when once fitted in their places, a new and entirely original difficulty presented itself. The colliery is situated on a dry chalk plateau; it is three miles to the nearest watercourse. The question of the disposal of the pumped water became crucial: as the pumping should only last a short time, a large capital outlay was not justified. The least expensive, but rather risky, method was adopted, and the result a failure. The pumps

mastered the water, but there was nowhere on the surface to get rid of it without letting it return to the pit.

Under the circumstances it was judged prudent to suspend progress in the other pit. In order to ascertain whether it was possible to avoid this mass of water by change of site, a third pit has been successfully carried down to the dry chalk of the zone of *Terebratulina gracilis*, which it entered at 300 ft., and penetrated to a depth of about 60 ft., having passed the dangerous level of 275 ft. without incident. A number of small feeders enter by the bedding planes, but the total amount is quite small and easily dealt with.

The drowning of the No. 1 Pit at Snowdown was a shock to the pioneers; but their faith has since been more than justified by the successful passage of the wet zone in all three collieries without trouble or expense.

VIII. THE LATEST BORINGS.

In 1907 it was found impossible to continue the Waldershare boring any deeper; it had attained a depth of nearly 3,000 ft., and proved three fine seams of excellent coal, excluding several seams of less than 3 ft. in thickness.

The Fredville boring unfortunately entered broken ground at about 1,800 ft., and had to be abandoned, but we have seen that this was not before it had amply justified its existence.

In order to prove the deeper ground thus missed at Fredville and partly in order to throw further light on the section at Tilmanstone, a third boring was started at Barfrestone, about half way between the collieries of Snowdown and Tilmanstone.

In order to prove the mineral areas acquired in the northerly direction, another boring was started near the village of Goodnestone, and as the company's boundaries gradually extended to the east, a fifth boring was undertaken near the village of Woodnesboro', at the foot of the Tertiary Escarpment. A sixth has been commenced at Walmestone, about a mile north-east of Wingham.

The geological information afforded by these three additional borings is of course of the very greatest value, but in view of possible competition it has been judged prudent to keep their results secret, and some time must elapse before the sections will be published.

IX. CONCLUSION

Thus we have traced the history of the discovery of the South-Eastern Coalfield from the primitive days of the early theorisers to the striking of good seams of coal at Dover in 1890. We have passed over the weary period of waiting and disappointment, the dark ages of Kent Coal, and we have discussed the Renaissance, the causes which led up to it and the momentous consequences which ensued.

We have seen how boring has succeeded boring, and finally how new collieries have been started, how they have successfully overcome the first difficulty, that of the chalk water, and how they are prepared to grapple with confidence with the difficulties which await them. Forewarned is forearmed, and the day is surely not far distant when coal will be brought to bank successively at the Tilmanstone, Guilford, and Snowdown collieries, and the beds of remarkable fire-clays will give birth to a new pottery industry, when we shall witness the realisation of that dream of prosperity which shall once more make Kent one of the wealthiest counties in England, and make good a thousand-fold the damage done by years of agricultural depression.

THE DUCTLESS GLANDS.

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INTRODUCTION.

THE idea of "secretion" has from the earliest period of physiology been associated with a "gland." The essential of a gland is a surface, more or less involuted, provided with "epithelial" cells, usually of a columnar or cubical shape, and characterised by the presence of granules of the substance to be secreted or its precursor. In a typical gland such as the submaxillary the primitive plan of an extended surface or a tubular depression has been so far superseded that the organ consists of a compact conglomeration of secreting cavities leading into "ducts" which become successively larger and larger till the chief "duct" of the gland is reached.

But some structures which resemble glands in their general characters and even in the nature of their cellular elements are found to possess no "alveoli" or secreting cavities and no ducts. Hence they have been called "ductless glands," or in Germany more usually "*Blutgefäßdrüsen*." The assumption was at once made that since these structures had the characters of glands they must "secrete." But since there was no communication with a free surface the hypothesis arose that in these cases the specific secretion is passed into the blood stream, and the process is termed "internal secretion." In some cases the material secreted by the ductless glands is passed not directly into the blood stream, but indirectly by way of the lymphatics. This is usually supposed to apply to the specific secretion of the thyroid gland.

The terms "ductless gland" or "*Blutgefäßdrüse*" were originally applied to a very varied group of structures, including the thyroids and parathyroids, the suprarenal capsules, the thymus gland, the pituitary body, the spleen, and the lymphatic glands. But some of these, viz. the spleen and lymphatic glands, have not a "glandular" structure, that is,

they do not consist of epithelial, "secreting" cells, and belong to quite a different category of organs, viz. the "hæmolymph" series (1). The structures usually included at the present time under the title of "ductless glands" are the thyroid gland (including the parathyroids), the suprarenal capsule consisting of "cortex" and "medulla," the pituitary body consisting of the "infundibular" or "nervous" portion and the "glandular" portion, the thymus gland, and the corpus luteum. It is believed that these "ductless glands" manufacture and pour, directly or indirectly, into the blood stream some substance or substances which are of service in the economy, either by supplying a need, or by destroying other substances which are needless or positively harmful. This last function, that of "Entgiftung," is usually ascribed to the thyroid, though in this, as in other cases, the two conceptions are not necessarily antagonistic.

It is perhaps desirable to point out at this stage that the term "internal secretion" has probably been used too generally and too confidently in many cases. Our knowledge of internal secretion is not to be compared in accuracy and definiteness with our knowledge of "external" or ordinary glandular secretion. Thus in the case of the submaxillary glands we can observe the various conditions, loaded or unloaded, of the gland cells. We can watch the flow of the secretion, and regulate it by stimulation of nerves. We can note changes in the volume and blood-supply of the gland concomitantly with the act of secretion. Finally, we can recognise an "enzyme" in the fluid secreted, and are familiar with its action on food as a process of digestion. Very different is the case, for example, of the medulla of the suprarenal capsule. Here nothing is known of any changes in the cells indicative of the act of secretion, and the very fact that any secretion is poured into the blood stream can only be shown by laborious and indirect methods. It must be confessed, in fact, that many of our conceptions of internal secretion rank little higher than plausible hypotheses.

But a typical gland having a duct and performing "external secretion" may possess according to modern views in addition the function of "internal secretion." This applies to the liver, the pancreas, the kidney, the testis, and the ovary (2). But in the present article we shall confine our attention to the internal

secretion of the ductless glands -viz. suprarenal, thyroid, pituitary, thymus.

(i) THE SUPRARENAL CAPSULES.

On slicing through the organ it is seen to be composed of two portions, a central or "medulla," and a peripheral or "cortex." The cortex is the greater in amount, and is in the view of many observers the true suprarenal gland, while the medulla is simply the "paraganglion suprarenale," a portion of the chromaffin tissue which has insinuated itself into the interior of the suprarenal capsule (3). One thing, at any rate, is clear, viz. that we have to deal with two separate and distinct structures. This becomes obvious from a study of the elasmobranch fishes, where the two portions are separate (4).

All our knowledge and nearly all our hypotheses in regard to the suprarenal capsule have reference to the "medulla" of the organ.

(A) *Effects of disease.*—Destructive disease—usually tuberculous—of the suprarenal capsules was shown by Addison (5) in 1855 to give rise to a train of symptoms constituting what is now known as Addison's disease. The symptoms are anaemia, profound asthenia, irritability of the stomach, and bronzing of the skin.

(B) *Effects of ablation.*—It seems to be generally admitted that the suprarenal capsules are organs essential for life, and that ablation of both capsules is always fatal within a few days (6). But it must be borne in mind that the operation is a very difficult one, and many of the deaths recorded must have been due to causes other than absence of the suprarenal capsules (7).

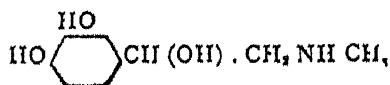
(C) *General physiological effects of extracts of the suprarenal capsules; Effects of subcutaneous injections.* After sufficiently large doses of suprarenal extract injected subcutaneously we get slowed muscular movements, paresis, and finally paralysis of the limbs (hind limbs always becoming affected first), bleeding from the mouth and nostrils, altered respiration, convulsions, and death. The paralysis is central. The effects are due to the medulla only; the cortex contains no toxic substance. The effects are specific for the suprarenal medulla, and not common to other gland extracts. The toxic material is easily eliminated in some way or other; this accounts for the large dose required,

and the ease with which recovery takes place. Idiosyncrasy plays a large part in the conditions. A partial immunity can be set up by giving doses insufficient to kill (8). Subcutaneous injections produce glycosuria in rabbits and dogs (9). The substance probably acts as a protoplasmic poison (10).

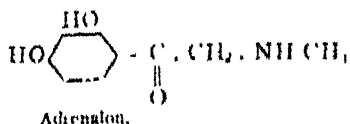
(v) *Special physiological effects of extracts of the suprarenal capsules; Effects of intravenous injections.*—The most important observation after that of Addison is the discovery of Oliver and Schäfer (11) in 1894 that extracts of the medulla of the capsules when injected into the circulation of a living animal give rise to an enormous rise of the blood-pressure, which is still greater when the vagi are cut or atropine is administered. The effect is a peripheral one. The flow of secretion from the salivary glands is increased (12). Repeated injections of adrenin into the auricular vein of the rabbit give rise to a degenerated condition of the wall of the larger arteries, especially of the aorta (arterio-sclerosis) (13). This effect is also produced by other blood-pressure-raising substances, such as digitalin and nicotine (14).

(f) *Mode and seat of action of suprarenal extracts.*—According to recent observations by Langley, in all cells two constituents at least must be distinguished: (1) substances concerned with carrying out the chief functions of the cells, such as contraction, secretion, the formation of special metabolic products; and (2) receptive substances, especially liable to change, and capable of setting the chief substance in action. According to this author the active substance of the suprarenal capsules produces its effects by combining with the receptive substance, and not on nerve-endings nor the chief substance (15). So that in this view the controversy as to whether adrenalin acts on muscle itself or on sympathetic nerve-endings is compromised by assuming that there is some material in cells originally under control of the sympathetic which is specially excited by adrenalin.

(f) *The chemical nature of the physiologically active substance.*—The active principle was first obtained in crystalline form by Takamine and Aldrich independently. Takamine calls his product *adrenalin*. It has the empirical formula $C_9H_{11}NO$, and the probable structural formula is:—



If adrenalin be oxidised we get a substance having the formula



This substance *adrenalon* has been prepared synthetically by Friedmann, Stolz, and Dakin, and the reduction product appears to be chemically identical with adrenalin in all respects except that it is optically inactive, while the natural body obtained from the suprarenal glands is laevorotary ("D" = -43).

On testing the physiological action of the synthetic (racemic) adrenalin, and comparing it with that of the natural adrenalin, it is found that the latter acts twice as strongly on the blood-pressure. From this it is inferred that dextro-adrenalin is devoid of action (16).

(g) *Theories as to the function of the suprarenal capsules.*—It has already been pointed out that the suprarenal capsule consists of two distinct organs, or perhaps it would be more correct to say of an organ (the cortex) and a portion of a tissue not primarily related to the organ (the medulla, "chromaffin" cells of Stilling and Kohn). As the embryology of these two constituent tissues is so totally distinct, it is probable that we ought to search for a separate function for each, and that we have no right to assume that these functions are of necessity intimately related to each other. All the well-ascertained facts, which appear to point to a definite physiological significance, apply to the medulla—the "chromaffin" cells, and not to the greater part of the dual body (cortex).

The two chief theories are (1) the "auto-intoxication" theory and (2) the theory of "internal secretion." The former was first clearly stated by Abelous and Langlois, who performed a series of extirpation experiments upon frogs. These authors found that the blood of animals dying as the result of suprarenal extirpation is toxic for other animals which have recently been deprived of their capsules. The symptoms caused by this blood are said to resemble those of poisoning by curari, and the conclusion reached by the French authors was that after suprarenal extirpation one or several toxic substances, the products of muscular metabolism, accumulate in the organism, and that the function of the glands is to remove or destroy these.

But, as pointed out by Schäfer, it is probable that the blood of an animal dying slowly as the result of any disease would be to some extent toxic, and that the toxic principles would more powerfully affect animals the resisting power of which had been lessened by a recent severe operation. Further, the suprarenals produce a material which has entirely different properties from those stated to be possessed by the blood of animals deprived of their capsules.

The "internal-secretion" theory is briefly as follows: The medulla of the suprarenal capsules is continually secreting into the blood an active material, adrenalin, or better, as Schäfer suggests, "adrenin," which is beneficial in maintaining the tone of muscular tissues, and particularly that of the vascular system. In this view the absence of the active substance in animals deprived of their suprarenal capsules, and in cases in the human subject in which both capsules were involved in disease, is the true cause of the symptoms which manifest themselves in these circumstances. It follows, according to this theory, that Addison's disease is due to suprarenal insufficiency—to an inadequate supply of the suprarenal secretion.

Although this theory of internal secretion is the one which is now most generally accepted, and which appears to have the balance of probability on its side, one is bound to admit that the direct evidence of the actual secretion of material into the blood streams is not very great. The blood from the suprarenal veins is stated to produce physiological effects greater than those obtained from other blood, and recently experiments have been performed under the direction of the present writer which seem to point definitely to a secretion from the medulla into the veins of the gland (17). But it must be admitted that, in view of our total ignorance of the functions of the cortex, and our doubt as to whether the two portions are related, the time is not ripe for any very definitely formulated views as to the functions of the suprarenal capsules.

(ii) THE THYROID AND PARATHYROID GLANDS

(A) *Effects of ablation and disease.*—Our knowledge of this subject is not based upon so secure a foundation as a perusal of modern text-books would lead one to suppose, and there can be little doubt that there has been in some directions undue haste in correlating clinical experience with the results of experimental

physiology. One of the most generally accepted among recent views may be stated as follows: As regards vital importance, the function of the thyroid gland is subsidiary to that of the parathyroids. Removal of all the parathyroids from an animal, even if the thyroid be left intact, invariably proves fatal within a short time, and this with typical nervous symptoms described under the name of "tetany." Removal of the thyroid, on the other hand, gives rise to an entirely different train of symptoms, stated to be those of "post-operative" myxœdema. This view assumes first of all that thyroid and parathyroid are separate organs with totally distinct functions. It further assumes that myxœdema in the human subject and post-operative myxœdema in animals are due to thyroid insufficiency, while the nervous symptoms which frequently occur after operations on animals are due to parathyroid insufficiency.

Unfortunately this theory, though devoid of complexity, is not supported by the evidence at our disposal. The extreme importance to life of the parathyroids is more than doubtful. Kishi (18) found that dogs and cats often die after operations on the thyroid, although the parathyroids be left behind. The present writer, in conjunction with W. A. Jolly (19), found further that removal of all four parathyroids was not necessarily fatal. We were furthermore unable to confirm some other statements which are commonly accepted. It cannot be truly said that either thyroids or parathyroids are essential for life, since it is frequently possible to remove either or both without causing death. The functional importance of the glands appears to differ very widely in different classes of animals. Rats and guinea-pigs do not seem to suffer at all as the result of extirpation. Monkeys only show transient nervous symptoms, and even these are sometimes wanting. Dogs and cats frequently, but by no means invariably, suffer severely and die. In foxes symptoms come on with remarkable rapidity, and death is correspondingly early. In no animal were we able to induce symptoms resembling myxœdema. Myxœdema, and probably also cretinism, must then, in our view, be due to causes more complex than simple thyroid insufficiency. When the thyroid is removed the parathyroids appear capable of functionally replacing it to a certain extent, and their histological structure changes accordingly.

Perhaps the point of most general interest in the above results

is that myxœdema was never induced even in monkeys. This result differs from that obtained by Horsley (20), Murray (21), and Edmunds (22), who state that it is possible by operation to induce myxœdema in monkeys. Our animals were kept at ordinary indoor summer temperature, and showed no symptoms which could be described as myxœdematous. In some cases it was impossible to distinguish the operated monkeys from the controls, and in a photograph of an operated animal and a normal one side by side the operated animal looked the healthier of the two. One monkey, it is true, suffered three days after the operation from slight muscular tremors, but completely recovered. In order to ascertain whether the fatal results, and especially those succeeding to nervous attacks, are due to injuries to surrounding structures and not to the absence of the glands, we performed some few experiments in which injury was done to structures in the neck without removal of either thyroids or parathyroids. No typical symptoms were observed in these cases, but in a later series of experiments one monkey died within twenty-four hours after the operation with what appeared to be typical tetany. It was found, however, *post mortem*, that the recurrent nerve and other structures had been included in one of the ligatures. There seems still to be a possibility that if a large number of experiments were performed and the necessary conditions were understood, it could be shown that some of the nervous symptoms are not due to glandular defect or insufficiency. This possibility is suggested by the fact that "tetany" may come on at variable periods from five hours to a week after operation.

Some of the animals suffered from catarrh, and one died from some laryngeal affection; and it seems probable that, as in the case of other animals, removal of the thyroid gland leaves monkeys in a condition in which they are less capable of resisting infection. We do not claim that all our monkeys were totally unaffected by the operation. They were, as a rule, perhaps somewhat quieter and more subdued than normal; but we do insist that the striking feature of myxœdema in man--viz. swelling of the subcutaneous tissues--was altogether wanting.

Of course it is open to insist that somewhere in the body of the above-mentioned animals there were accessory thyroids or parathyroids which sufficed to maintain life after the removal of

the main organs. We can only reply that a most careful search for such bodies has always been made, and that in the absence of positive evidence their existence in any particular case can only be purely conjectural.

The operation of removing parathyroids without injury to the thyroid is so difficult that in most cases it may be considered an impossible one. It is to be noted, however, that removal of the parathyroids, included of course in the complete operation, has not proved fatal (in monkeys), so that if simple parathyroidectomy is a fatal operation, it follows that removal of parathyroids alone is a more dangerous proceeding than extirpation of the whole apparatus. This has in fact been alleged by some observers.

In 1875 Gull described a disease which he called "cretinoid cachexia." Four years later Ord proposed the name "myxœdema," which has since prevailed. In 1882 and 1883 Reverdin (23) and Kocher (24) observed symptoms very similar to those of myxœdema in some of their patients from whom they had removed the whole of the thyroid. Hence Reverdin called the condition "operative myxœdema" (Kocher's designation was "cachexia strumipriva").

In 1884 Horsley resumed the subject of experimental extirpation in animals which had been commenced by Schiff in 1856. Although, as will be gathered from the foregoing account, the present writer has not been able to confirm the statements of Horsley and his successors, Murray and Edmunds, that removal of the thyroids from monkeys induces myxœdema, yet it must be admitted that these experiments stimulated inquiry, both experimental and clinical, and led to the employment of thyroid given by the mouth in the treatment of myxœdema. This method was first employed by Mackenzie (25) and Howitz (26).

It must be noted that in the record of the Swiss surgeons, "operative myxœdema" followed only in a certain number of total removals of the thyroid gland. It would be interesting to repeat the extirpation experiments, employing the anthropoid apes for the purpose.

(B) *The question as to the relationship between thyroids and parathyroids.*—Some of the earlier observers looked upon the parathyroids as simply undeveloped portions of the thyroid tissue. The actual transformation of parathyroid into thyroid tissue has been denied by the majority of investigators. But

Kishi definitely states, as did the older observers, that the parathyroids are embryonic thyroids. The present writer, in conjunction with W. A. Jolly, found that parathyroids left behind after thyroidectomy can under certain conditions develop in the direction of thyroid tissue, and a functional replacement also takes place.

The two structures are derived from very similar sources, and even in the fully developed state there is no fundamental difference between their constituent cells. The intervesicular tissue of the thyroid is practically identical with parathyroid, and parathyroid has only to contain colloid vesicles in order to constitute itself thyroid. This view has recently been confirmed from the standpoint of comparative anatomy by Forsyth (27), and further evidence will soon be published from the Physiological Laboratory at Winnipeg. It is further to be noted that in structures which are topographically parathyroids we sometimes find colloid vesicles, especially in the human subject. It is a matter of no importance to the present discussion whether these are normal or pathological. It is impossible, then, to avoid the conclusion that thyroids and parathyroids are morphologically and functionally very intimately related. Perhaps it is not going too far to say that we should look upon the two structures as forming one apparatus.

(c) *Chemistry of the thyroid.*—The question naturally arises, what is the chemical nature of the active principle of the thyroid gland? What is the substance, or what are the substances, the absence of which causes such serious metabolic disturbances in some animals? We may say at once that there is at present no satisfactory answer to the question. There have been a large amount of work and much discussion on the subject. Baumann discovered the presence of iodine in the thyroid glands, and prepared a substance which he called "thyroidin," and which he thought was the active principle of the gland. But the "activity" of the substance in neutralising the ill effects of thyroidectomy is disputed, and there are other objections to Baumann's theory. Iodine is absent from the thyroids of some animals. Baumann himself states that while the thyroid of a dog fed upon Spratt's dog biscuits contains iodine, this element is absent after a meat diet. In the ox, horse, and pig, iodine may be absent or may be present in the merest traces; further Baumann admits that iodine cannot always be found in the

human thyroid. The presence of iodine may not after all be a matter of supreme significance for the function of the thyroid. It is at any rate so problematic that the whole matter requires careful investigation. No chemically pure substance has been isolated from the gland (28).

(D) *Influence of the thyroid upon metabolism.*—Metabolic processes are distinctly increased by the administration of thyroid substance. There is at first no influence on proteid metabolism, but an increase of nitrogenous excretion from increased elimination of nitrogen-holding extractives already present in the body. The body fat is first used up. After a certain period, however, the proteid is also attacked (29).

(E) *Theories as to the function of the thyroids and parathyroids.*—The most generally accepted view is that the thyroid gland furnishes in the form of an internal secretion some substance or substances which are in the human being and some other animals, essential for the maintenance of health. The results of extirpation in human beings and in some animals, the effects of thyroid disease in the human subject (cretinism and myxœdema), combined with the beneficial effects of the administration of thyroid extracts, are strong arguments in favour of this view.

Nothing positive is known as to the functions of the parathyroids considered separately from the thyroids.

(iii) THE PITUITARY BODY

(A) *Structure and development.*—The pituitary body consists of three portions: (1) The anterior lobe proper; (2) an intermediate portion; (3) the nervous portion.

The epithelial portion of the pituitary body is differentiated into two distinct parts: an anterior lobe proper, consisting of solid columns of cells, between which run wide blood-channels; and an intermediate portion, which lies between the anterior lobe and the nervous tissue of the pituitary, forming a closely fitting investment of the latter.

The anterior lobe contains cells which are either clear or granular. The intermediate portion consists of finely granular cells arranged in layers of varying thickness closely applied to the body and neck of the posterior lobe and to the under surface of adjacent parts of the brain. Colloid material occurs between the cells of the pars intermedia.

The nervous portion of the pituitary body is made up of neuroglia cells and fibres. Ependyma cells line the central cavity in the cat, and send long fibres forwards and upwards towards the brain, most of which terminate in the outer part of the neck. There are no true nerve cells, and the nerves supplying the pituitary probably reach it through sympathetic fibres accompanying the blood-vessels. The nervous portion is invaded to a large extent by the epithelial cells of the pars intermedia. Columns of epithelial cells grow into it, especially in the region of the neck, and islets of these cells are frequently found throughout the posterior lobe.

A substance histologically resembling the colloid of the thyroid gland occurs in the nervous portion of the posterior lobe. It appears to be a product of the epithelial cells, and to be carried by lymphatics into the third ventricle of the brain.

Development of the pituitary body begins very early in embryonic life. In mammals the epithelial portion is derived entirely from the ectodermic wall of the buccal invagination known as Rathke's pouch. Its origin is single and mesial. The epithelium is early distinguishable into two parts. One of these, the intermediate part, is closely adherent to the wall of the cerebral vesicle; the cells are clear, and tend to form colloid. The other portion of the buccal epithelium gives rise to the anterior lobe proper. Its cells are granular, and form solid columns separated by blood-channels.

The infundibulum is an invagination of part of the wall of the thalamencephalon, which is adherent to the anterior and upper wall of Rathke's pouch. It therefore possesses an epithelial covering derived from the latter. The infundibular process grows backwards, and, in the cat, retains its central cavity. It is lined by ependyma cells, which during development become elongated, so that ependyma fibres run obliquely in its neck. The posterior lobe of the pituitary is, from the first, a composite structure of epithelium of the pars intermedia and of neuroglia and ependyma, and the relations between the two tissues become more and more intimate (30).

(B) *Physiological action of extracts of the pituitary body.*—There are two substances contained in an aqueous extract having opposite effects (pressor and depressor) upon the blood-vessels; but with the first injection the depressor substance is over-

powered by the pressor, while with subsequent injections these conditions are generally reversed (31).

Besides the pressor and depressor constituents of extract of the infundibular part of the pituitary body, this organ yields a substance soluble in water and not destroyed by boiling, which acts specifically upon the kidney, producing, along with dilatation of the renal vessels, an increase of secretion from the tubules. According to Schäfer and Herring (32), the diuretic activity of the substance formed by and contained within the gland is greater than that of any diuretic in the pharmacopœia; and assuming that the pituitary is an internally secreting gland, it is probable that the main purpose of its secretion is ancillary to the function of the renal organs.

Extracts of the posterior lobe of the pituitary body of the ox produce a distinct dilatation of the pupil of the enucleated frog's eye (33).

It is to be noted particularly that all these effects are obtained from the posterior—the "nervous"—part of the pituitary; and the pituitary of the elasmobranch fishes, which has no nervous portion, does not yield active extracts (34).

(c) *Theories as to the functions of the pituitary body.*—The theory held by Schäfer and Herring is that the internal secretion of the nervous part of the gland controls the renal functions, both by the effects of its secretion upon the vascular system in general, as well as by a direct influence exerted by one of its constituents upon the renal cells and upon the renal circulation in particular.

It has been supposed that the pituitary may act vicariously for the thyroid gland.

The condition of acromegaly is frequently found to be associated with tumours of the pituitary body; and it is interesting, as pointed out by Schäfer and Herring, that cases of tumour of the pituitary have been recorded in which polyuria was a prominent symptom.

(iv) THE THYMUS GLAND

The thymus gland may be classed in a general way with the lymphoid tissues. It is well developed in late foetal life and in the new-born, and reaches its highest development about the end of the second year, although it occasionally retains a considerable size in the adult. After the second year it atrophies

and becomes fatty, but it always contains remnants of its lymphoid elements.

The thymus is derived from the entodermal portion of the branchial clefts, and is at first epithelial in structure. The precise mode in which the transformation into adenoid tissue occurs is still under discussion. There can be little doubt that the concentric corpuscles are the remains of the original epithelium.

Hypertrophy of the thymus is often found *post mortem* in cases of sudden death, most particularly in infants, but occasionally also in adults.

Nothing whatever is known of the functions of the thymus. Extirpation gives rise to no ill effects (35).

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THE CHEMISTRY OF CHLOROPHYLL

BY S. B. SCHRYVER

IN spite of the large amount of labour devoted to the subject, both by botanists and chemists, but little progress has been made until very recent times, in the elucidation of the true chemical nature and constitution of the green colouring matters of plants. This is due to the many technical difficulties in which the experimental investigation abounds; the small yield obtainable, their labile nature, and the difficulty of separating the green pigments from fats and other colouring matters by which they are always accompanied, may be considered as the chief causes which have impeded the knowledge of the chemistry of chlorophyll.

The fact that green leaves yield up their pigment to fats and oils was discovered in 1682 by Nehemiah Grew, and the solutions thus obtained were the subject of several investigations during the course of the eighteenth century and the earlier years of the nineteenth century, notably by Rouelle, Meyer, Fourcroy, Berthollet, Sennebier, Proust, and Vauquelin. Their discoveries were, however, of subsidiary importance, and require no detailed consideration in this place.

The name "chlorophyll" was first given to the green pigment by Pelletier and Caventou in the year 1817; these investigators recognised that the alcoholic extract was not of homogeneous nature, but consisted of a mixture of several substances. Berzelius attempted to separate the constituents of the mixture, and obtained, by treating the alcoholic extract of plants with alkali, a water soluble constituent, which is to-day known as alkachlorophyll.

The physical properties of chlorophyll solutions were also the subject of numerous investigations during the earlier half of the nineteenth century, notably by Brewster and Stokes, who examined and plotted the absorption spectra, and made various observations on the phenomenon and fluorescence of the solutions.

Valuable additions to the knowledge of chlorophyll were made later by Frémy, Timiriazeff, Kraus, Tschirsch, Hansen, and others, but the principal advances are due to the labours of Hoppe-Seyler, Schunck and Marchlewski, and to Willstätter and his pupils. The three series of researches by these investigators require some detailed consideration, during the course of which the influence of the discoveries of the earlier observers will be discussed.

INVESTIGATIONS BY HOPPE-SEYLER

In 1879, Hoppe-Seyler isolated from grass a crystalline substance which he regarded as a derivative of the original green pigment of plants, and to which he gave the name of *chlorophyllan*. He obtained this product in the following way: The grass was first extracted with ether several times with the object of separating the fats, and then with boiling alcohol; the extract thus obtained was then employed for the extraction of a second portion of grass, which had also been previously treated with ether. The concentrated extract thereby obtained, deposited crystals on standing. The filtrate from these crystals was then evaporated, the residue washed with water, and the part insoluble in water extracted with ether. This ethereal solution deposited on standing crystals of chlorophyllan, which could, after washing with the cold solvent, be purified by recrystallisation from hot alcohol. The product analysed was purified by repeated alternate recrystallisations from ether and alcohol, and was found to contain both magnesium and phosphorus.

By treatment of chlorophyllan with a potassium hydroxide at 290°, an acid was obtained, to which Hoppe-Seyler gave the name of *chromatinic acid*, and from which he prepared a barium salt, the composition of which corresponded with the formula $(C_{21}H_{28}O_5)_2 Ba$. By treating the salts or the acid itself with excess of mineral acids, a decomposition product of chromatinic acid was obtained, which dissolved in acid water with a blue-purple colour; on account of the similarity of the light-absorption of the solution thereby obtained, to that of the blood-pigment derivative described but a short time before, and prepared by Hoppe-Seyler by the action of acids or reducing reagents on hæmin, and called by him hæmatoporphyrin, the new substance from chlorophyll was designated *phylloporphyrin*. The intimate chemical relationship between phylloporphyrin

and hæmatoporphyrin was only completely demonstrated some years later.

By more gentle treatment of chlorophyllan with alcoholic potassium hydroxide, another acid was obtained, which could be readily separated from other products by taking advantage of the fact that its potassium salt was soluble only with difficulty in alcohol; to prepare it chlorophyllan was boiled for one hour with alcoholic potassium hydroxide; carbon dioxide was then passed through the solution, and a precipitate consisting chiefly of the potassium salt of the new acid and of potassium carbonate was obtained; this was dissolved in water, and the barium salt of the acid precipitated by means of barium acetate; from this salt the free acid was obtained by the addition of acetic acid. It was called by Hoppe-Seyler *chlorophyllanic acid*. In addition to the insoluble barium salt, a soluble salt was also obtained, which was shown to be barium glycerophosphate; and Hoppe-Seyler concluded that chlorophyllan was a derivative of lecithin, an opinion which was generally held until quite recently, when it was shown to be erroneous by the investigations of Willstätter, to be described below.

Chlorophyllan was held by Hoppe-Seyler himself to be probably a changed product of the original pigment contained in plants. That this view was correct was subsequently shown by Tschirsch, who found that the plant acid caused a decomposition of the original chlorophyll, leading to the production of chlorophyllan. The actual nature of the change was demonstrated afterwards by Willstätter (see below).

The most important discovery due to Hoppe-Seyler is perhaps that of phylloporphyrin; it was not prepared by him in a pure condition, but it was the first product obtained which indicated a chemical relationship between the green pigments of plants and the blood.

RESEARCHES BY SCHUNCK AND MARCHLEWSKI

The investigations to be described under the above heading were commenced by Schunck alone, who published a preliminary account of his results in the *Proceedings of the Royal Society*; he was joined afterwards by Marchlewski, and the results of the conjoint work appeared in *Liebig's Annalen*; in the latter publications the full experimental details are given.

Schunck and Marchlewski investigated in a systematic manner the action of acids and alkalis on chlorophyll.

Phyllocyanin and Phylloxanthin.—By passing hydrochloric acid through an alcoholic solution of chlorophyll, Frémy had previously obtained two products, of which one, to which he gave the name *phyllocyanin*, was soluble in acids, by means of which it could be extracted from ethereal solution, whereas the other, which he called *phylloxanthin*, was insoluble in acids, and remained in ethereal solution, when the latter containing the mixture of the two bodies was shaken with hydrochloric acid. Frémy assumed that phylloxanthin and phyllocyanin were derivatived by the scission into two components of the pigment originally in the plants. The observations of the French chemist served as the starting-point of the investigations of Schunck, who succeeded in obtaining the acid soluble phyllocyanin in crystalline form. He also attempted to obtain a pure phylloxanthin and, with Marchlewski, succeeded in showing that the product soluble in ether as obtained by Frémy contained, besides a decomposition product of chlorophyll, another plant pigment, viz. carotin. The method employed for the preparation of the acid decomposition products was as follows: Fresh grass was extracted with hot alcohol, and the filtered solution allowed to stand for some days; a deposit was thereby formed of wax and other plant substances. The filtrate from these was then saturated with hydrochloric acid, and a separation of an almost black mass took place. This precipitate contained the greater part of the phylloxanthin and phyllocyanin; it was almost completely soluble in ether, and the separation of the two substances it contained was effected by shaking the ethereal solution with an equal volume of concentrated hydrochloric acid. After separation of the aqueous and ethereal layers, the latter was again shaken with fresh quantities of acid, and the process repeated until the acid solution remained nearly colourless. The combined acid extracts were allowed to stand in open basins to evaporate off the ether, and then diluted with water; a precipitate of phyllocyanin was thereby obtained, which was purified by repeated recrystallisations from glacial acetic acid.

Phyllocyanin thus obtained formed a dark blue mass, resembling indigo, of microscopic crystals, which decomposed between 160° and 180° without melting; it was insoluble in water

and light petroleum, but soluble in alcohol, ether, chloroform, benzene, aniline, and carbon disulphide. It readily formed double salts when treated with the acetates of heavy metals; the copper derivative, which was beautifully crystalline, had a composition corresponding with the formula $C_{55}H_{71}O_{17}N_6Cu_2$.

The phylloxanthin was prepared from the ethereal solution after separation of the phyllocyanin by acid, by allowing it to evaporate in flat basins. The residue was dissolved in chloroform and to the solution thus obtained, several times the bulk of alcohol was added. A precipitate containing the greater part of the phylloxanthin was thereby obtained. This was dissolved in hot glacial acetic acid, from which it is deposited on cooling. The product thus obtained was subjected to a somewhat complicated purification process, but in spite of this it could not be obtained pure and free from fatty admixtures. It was a dark green mass without crystalline structure, soluble in hot, and but slightly soluble in cold alcohol, and soluble in most organic solvents. Its absorption spectrum was similar to that of phyllocyanin. By the action of strong acids in the cold it can be converted into this substance.

Alkachlorophyll.—The action of alkalis was also the subject of investigation by Schunck (afterwards with Marchlewski).

Senebier had noticed that the action of alkalis on chlorophyll solution produced no marked change in the colour, and Berzelius adopted the view that no chemical change was produced by the treatment. Hansen, who made a detailed study of the subject, expressed the opinion that, by the action of alkalis, a saponification took place, with the elimination of a fatty acid which was in combination with the pigment. By this treatment, according to his opinion, "pure chlorophyll" was obtained.

Schunck, and afterwards Schunck and Marchlewski, prepared by the action of alkalis a product which was called *alkachlorophyll*, and which, they concluded, was not the original green pigment, but a derivative of the same. They prepared an alcoholic extract of chlorophyll, to which, after standing for some hours to allow the separation of fats and resins, they added sodium hydroxide. The mixture thus obtained was boiled for some hours. It was then allowed to stand, filtered from the resinous material, and then treated with carbon dioxide. A precipitate was obtained containing the sodium salt of alka-

chlorophyll. (Compare Hoppe-Seyler's chlorophyllanic acid.) This was dissolved in water, and the solution then saturated with common salt; the sodium salt of the pigment separated from solution. It was separated from salt by alcohol, and from the soaps which contaminate it by washing it with a mixture of alcohol and ether. Finally, it was dissolved in water, the aqueous solution made slightly acid with acetic acid, and the precipitated pigment extracted with ether. It was purified by dissolving in ether and precipitating from this solution by light petroleum, in which it is only very slightly soluble. In this way a product was obtained in the form of a dark greenish-blue mass, insoluble in water, with difficulty soluble in alcohol and ether, and insoluble in light petroleum. It possessed a characteristic absorption spectrum. Its chemical constitution corresponded with the formula $C_{30}H_{40}O_7N_4$ or $C_{30}H_{38}O_7N_4$. It dissolved easily in alkalis, and gave a sodium salt which was only with difficulty soluble in alcohol.

Phyllotaonin.—Alkachlorophyll dissolved readily in concentrated hydrochloric acid with no apparent change; when, however, this acid solution was extracted with ether, the latter took up a pigment, which on spectroscopic examination revealed the presence of another substance, which Schunck and Marchlewski had previously obtained by the action of acids and alkalis on phylloxanthin and phylloeyanin. This product had been called by Schunck *phyllotaonin*, and the alkyl esters of this substance were readily obtained from alkachlorophyll, by dissolving it in strong hydrochloric acid, and throwing the acid solution into boiling methyl or ethyl alcohol.

This phyllotaonin was obtained, therefore, from the products of the action both of acids and of alkalis on chlorophyll. From the products of the alkali reaction it could be obtained by treatment with acids, and from the products of acid reaction, by treatment either with acids or alkalis. On account of the interesting degradation product obtained from it, phyllotaonin deserves a more detailed consideration. It could be readily obtained from the crude alkachlorophyll solution. The chlorophyll solution, after boiling with alkali, was saturated with hydrochloric acid gas. The green solution became somewhat lighter, and assumed afterwards a purple tint. On allowing it to stand for about ten days, beautiful needles in star-shaped aggregates, appearing purple in reflected light, and brown in

transmitted light, separated out, which were purified by dissolving in chloroform and mixing the solution thus obtained with several times its volume of alcohol. When a methyl alcoholic solution was employed a methyl ester of taonin was obtained, and with ethyl alcohol an ethyl ester. Both products were very similar in appearance and properties. They were but slightly attacked by aqueous alkalis, but were readily saponified by alcoholic solutions. In this way phyllotaonin itself could be prepared. It could also be obtained by evaporation of phyllocyanin or phylloxanthin with alkalis and acids, and purifying the product by means of chloroform and alcohol in the way already described. It formed crystalline scales which were opaque to transmitted light, and appeared steely-blue in reflected light. It was readily soluble in alcohol and ether, and was also soluble in benzene, carbon disulphide, chloroform, and aniline. Its analyses corresponded with the formula $C_{40}H_{30}O_6N_6$. On distilling with zinc-dust it gave a pyrrol-like substance. An acetyl derivative could be obtained from it, although no alkyl esters could be prepared by directly treating it with alcohols and hydrochloric acid.

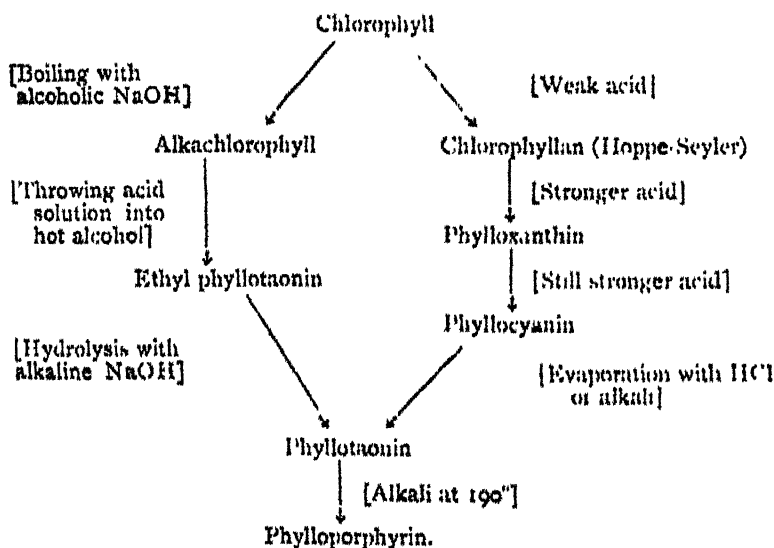
Phylloporphyrin.—The most interesting product, perhaps, obtainable from phyllotaonin was that prepared by Schunck and Marchlewski, by heating it in sealed tubes for some hours with alcoholic potassium hydroxide at 190° . On acidifying the mixture thus obtained, and extracting, after acidification, with ether, the latter assumed a purple-red colour, and deposited on evaporation dark violet-coloured crystals mixed with an amorphous brown substance. The alcoholic solution of the product thus obtained gave on treatment with alcoholic zinc acetate a beautiful red crystalline, zinc salt. This was dissolved in hot alcohol, and the solution acidified with hydrochloric acid and thrown into water. The mixture was then extracted with ether, which on evaporation deposited carmine-red crystals which could be purified by recrystallisation from alcohol. This substance was identical with that obtained by Hoppe-Seyler from his chlorophyllan by heating with potassium hydroxide, although it was not prepared by him in a pure condition. It was designated *phylloporphyrin*, and the name was retained by Schunck and Marchlewski for the product obtained by them from phyllotaonin.

In its pure condition it was a beautiful dark violet crystalline

substance, soluble only with difficulty in alcohol and ether, but somewhat more easily in chloroform. It dissolved very readily in mineral acids and in acetic acid, giving violet solutions of fairly stable salts; from solutions in mineral acids it could not be extracted by ether. It also possessed a somewhat acidic character, although this is not so marked as its basic character. It gave, as already noticed, a characteristic zinc salt. Its analysis agreed fairly with the formula $C_{55}H_{70}O_4N_4$.

Great interest attaches to this body on account of its chemical relationship with the derivative of the blood-pigment hæmatoporphyrin which will be discussed again later.

These experimental methods of preparing the above-described derivatives, together with their chief properties, have been discussed in some detail, in order that a comparison might be instituted between them, and the series of products prepared later by Willstätter and his fellow workers. The relationship of the products prepared by Schunck and Marchlewski is indicated in the accompanying scheme :



RESEARCHES BY WILLSTÄTTER AND HIS PUPILS

Willstätter's earlier researches were directed towards the study of the earlier decomposition products of chlorophyll obtained by the action of acids and alkalis. In the investigations

of Schunck and Marchlewski, neither the alkachlorophyll nor phylloxanthin was obtained in a satisfactory crystalline form, nor could either lay claim to anything more than approximate purity. Nor did the earlier researches throw any light on the changes undergone by the original pigment when treated with alkalis and acids. Furthermore, it is not by any means proved that the crystalline products obtained by the earlier investigators were homogeneous products, in spite of their crystalline form.

Phytochlorins and Phytorhodins.—Willstätter, by the introduction of a new method of purification, has shown that by the action of acids and alkalis on chlorophyll a variety of products can be obtained, which can be separated from one another by a fractional extraction of their ethereal solutions by graded strengths of acids. By varying the conditions of experiment, different quantities of these products could be obtained. Two main groups of products were obtained. One of these groups consisted of products which yielded in indifferent solvents, olive green to green solutions, whereas the other, obtained mainly from alkachlorophyll, by the action of alcoholic hydrochloric acid, was composed of the substances which in acid solutions were of blue to green colour, while in neutral solvents they were a brilliant red. The products belonging to the former of these groups were termed the *phytochlorins*, whereas the latter were called *phytorhodins*. The substances belonging to these classes were insoluble in water, but more or less soluble in organic solvents. They were weak acids, soluble in alkalis, even in ammonia, and bicarbonate, but could be quantitatively extracted from such solutions by ether. The various members of the groups could, however, be differentiated by their varying basic characters. Some of them could be readily extracted from their ethereal solution by means of weak hydrochloric acid, and were consequently of somewhat marked basic character, whereas others required acid of much higher concentration for their extraction; in these the basic character was consequently much less marked. Upon these differences in the basicity, Willstätter's method of separation was founded. As an example of the employment of the method, the following experiment may be quoted: 200 grams of extract obtained from 9 kilos of stinging-nettle leaves were dissolved in 2 kilos of 96 per cent. spirit, and so much sodium hydroxide solution added as to make a 2 per

cent. solution. The mixture was heated for a quarter of an hour with a reflux condenser and diluted with water, neutralised with hydrochloric acid, and extracted with ether; the ethereal solution was then freed from alcohol by repeated washing with water. The ethereal solution was then extracted with 17 per cent. hydrochloric acid, and in this way the very weak bases and indifferent substances separated from the main products of the reaction. The hydrochloric acid extract was then diluted with water, so as to have an acid content of 13 per cent., and extracted with ether to separate the last portions of the weak bases. The acid solution was then neutralised and extracted with ether, and a solution was obtained which contained two chief products, designated phytochlorin *a*, and phytochlorin *b*. The ethereal solution was then extracted several times with 3 per cent. hydrochloric acid, which is just strong enough to extract the more basic product phytochlorin *b*, whilst the less basic product remained in solution. On neutralising the 3 per cent. acid solution and extracting with ether, the product phytochlorin *b* was obtained nearly pure. The original ethereal solution, after the extraction of the phytochlorin *b*, was then shaken with $4\frac{1}{2}$ per cent. hydrochloric acid solution; this was not quite strong enough to extract the weaker base phytochlorin *a*, but sufficient to wash out the last traces of the product *b*. The ethereal solution was then extracted with 6 per cent. acid, which extracted fully the weaker base. By the repetition of this extraction, *i.e.* by extraction of the ethereal solution of phytochlorin *b*, with $3\frac{1}{2}$ per cent. acid, and of phytochlorin *a* with 6 per cent. acid after a previous extraction with $4\frac{1}{2}$ per cent. acid, the two products could be obtained pure.

By dissolving phytochlorin *a* in concentrated alcoholic potassium hydroxide, and heating the solution thus obtained for a short time, it is converted quantitatively into phytochlorin *b*; both products, furthermore, undergo change on standing in acid solution, being converted chiefly into a mixture of two other products, designated respectively phytochlorin *c* and phytochlorin *d*. All these are crystalline products which can be distinguished from one another by differences in the crystalline forms, in the colours of their solutions, and degrees of solubility in organic solvents. Their chief properties are summarised in the accompanying table (Table I.).

TABLE I

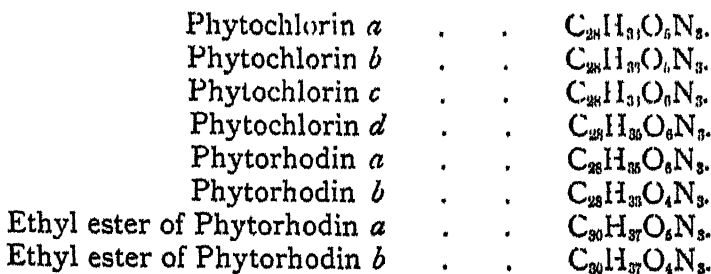
	Phytochlorin <i>a.</i>	Phytochlorin <i>b.</i>	Phytochlorin <i>c.</i>	Phytochlorin <i>d.</i>
Per cent. HCl, which readily ex- tracts base from etheral solution	7	4	1.5	0.5
Crystallises from hot alcohol	Needles united in rosettes	Plates	Spindle-shaped crystals	Prisms with twined forms
Colour of ethereal solution	Olive-green	Olive-green	Green - olive tinge	Pure green
Colour of solution in hydrochloric acid	Bluish not fluo- rescent	Bluish-green not fluorescent	Blue, fluorescent	Violet - blue, strongly fluo- rescent
Solubility in methyl alcohol	With difficulty in cold; easily in hot	With some diffi- culty in cold; easily in hot	Soluble easily in cold	Very readily in cold
Solubility in ben- zene	With difficulty in cold; easily in hot	With great diffi- culty in cold; somewhat in hot	Almost insoluble in cold; with some difficulty in hot	Almost insoluble in cold; with great difficulty in hot

The phytorhodins were obtained from an alkachlorophyll which was prepared by treatment of chlorophyll extracts with cold alcoholic potassium hydroxide. Two fractions were thereby obtained, one of which readily separated as an insoluble potassium salt from the alcoholic solution (compare results of Hoppe-Seyler and Schunck and Marchlewski), whilst the other remained in solution; this fraction was precipitated in the form of a calcium salt by the addition of alcoholic calcium chloride to the filtrate from the insoluble potassium salt. These fractions were then heated with ethyl-alcoholic solution of hydrochloric acid (20 per cent.) for several hours, and a series of the so-called phytorhodins was obtained, chiefly in the form of the ethyl esters. From the potassium salt the chief product was phytorhodin *a*, whilst from the calcium salt the chief product was phytorhodin *b*. These were accompanied with smaller quantities of other phytorhodins from which they could be separated by the method of fractional extraction with graded strengths of acids by the method already described. It is unnecessary in this place to give the details of the methods; the properties of the chief products isolated which can be distinguished from one another by the differences in crystalline form and the colours of their solutions are given in the following table:

TABLE II

	Phytorh- odin <i>a.</i>	Phytorh- odin <i>b.</i>	Ethyl ester of Phyto- rhodin <i>a.</i>	Ethyl ester of Phyto- rhodin <i>b.</i>	Phytorh- odin <i>c.</i>	Phytorh- odin <i>f.</i>
Per cent. IICI, which readily extracts the substance from ethereal solution	6.5	9	9.5	11	3	11
Crystallisation from ether	Needles in tufts	Tables, rhombic	Prisms	Tables	Needles in tufts	Prisms in rosettes
Colour of ether- eal solution	Carmine- red	Purple-red	Carmine- red	Purple-red	Purple-red	Deep red
Colour of solu- tion in IICI	Bluish-green weakly fluorescent	Bluish-violet fluorescent	Greenish- violet weakly fluorescent	Bluish- violet fluorescent	Violet greenish- blue nuance weakly fluorescent	Green. Not fluorescent
Solubility in ether	Fairly solu- ble	Fairly solu- ble	Very easily soluble	Easily soluble	Fairly soluble	Very slight- ly soluble
Solubility in cold alcohol	Fairly solu- ble	Soluble with diffi- culty	Easily soluble	Soluble with diffi- culty	Soluble with diffi- culty	Dissolves with some difficulty

None of the products of either the phytochlorin or of the phytorhodin series could be identified with certainty with the derivatives described by Schunck and Marchlewski. Phytochlorins *a* and *b* are very similar to phyllotaonin; and some of the phytorhodins are similar to phylloporphyrin; the phytorhodin esters are similar in many respects to the phyllotaonin esters. The probability exists that the methods employed by Schunck and Marchlewski, depending as they did on repeated recrystallisation only, were insufficient to effect complete separation of the mixtures. The chief products of Willstätter approximately correspond in their analyses with the following formulae:



Some of these products appear to be stereoisomers.

These results indicate that by the action of alkalis on chlorophyll, followed by that of acids, a complex series of products is obtained. The introduction by Willstätter of a method of fractional separation marks a great advance in the study of the chemistry of chlorophyll.

Chlorophyllin.—Investigations were next directed towards the preparation of a pure chlorophyll, and to the study of the actual change wrought by acids and alkalis on the original plant-pigment; as the result of which two facts of prime importance were discovered. The first of these was the fact that magnesium is an essential constituent of chlorophyll, and was readily eliminated from the molecule by the action of even weak acids; it was not, however, readily removed by the action of alkalis. The second important fact discovered was that chlorophyll, in part at any rate, is the ester of an unsaturated alcohol called by Willstätter *phytol*; by treatment with alcoholic solutions of caustic alkalis, the phytol ester readily undergoes saponification. It was also found that the hypothesis of Hoppe-Seyler, that chlorophyll is a lecithin derivative, is incorrect; phosphorus is only an accidental contamination of chlorophyll preparations. Preparations made from grass and stinging-nettles contained either traces or no phosphorus at all. The chief difficulty in this part of Willstätter's investigations was due to the admixture of chlorophyll with other plant-pigments, notably xanthophyll or carotin, which will be referred to in greater detail below.

Attempts were first made to prepare a purified chlorophyll solution, freed as completely as possible from other pigments. This was accomplished in the following way: A double methyl-alcoholic extract was mixed with an equal volume of light petroleum (B.P. 35-50°) and water added, so that the mixture separated into two layers; the petroleum layer was then washed with diluted methyl alcohol, and afterwards with water, and the solvent distilled off, after drying the solution with anhydrous sulphate of sodium.¹ The crude chlorophyll thus obtained was further purified by redissolving in methyl alcohol and treatment of the solution with light petroleum in the way just described. Another method of freeing chlorophyll from the accompanying pigments was to mix a solution in acetone with

¹ The object of this operation was to separate the other pigments accompanying chlorophyll. It was shown in subsequent researches (see below) that it is impossible to separate carotin from chlorophyll in this way.

water; a colloidal solution is thereby obtained: from this ether extracts carotin, but not chlorophyll. After this extraction, calcium chloride was added to the colloidal solution; after this addition the chlorophyll was readily dissolved by ether. The crude chlorophyll prepared from various sources by these methods contained in some cases no phosphorus at all, and in others from 0.0117 to 0.44 per cent.; the magnesium content, however, was generally about 1 per cent.

Attempts were also made to prepare the first and least changed derivative obtained from chlorophyll by the action of alkali. It was found that by the action of alcoholic potassium hydroxide in the cold a product was obtained which contained only 7 per cent. of nitrogen, whereas the alkachlorophyll of Schunck and Marchlewski contained more than 11 per cent. The substance prepared by the action of cold alcoholic caustic alkali solutions was designated *chlorophyllin*. (This name was originally suggested by Timiriazeff.) It was prepared by adding a methyl-alcoholic solution of potassium hydroxide to an alcoholic double extract of leaves, dissolving the salt which separated out, in water, and extracting the aqueous solution with ether. By this means, impurities such as carotin, which accompany the potassium salt, could be separated, the sodium salt of the acidic chlorophyllin remaining in the aqueous solution. From this it can be obtained by just neutralising with phosphoric acid, then acidifying with dihydrogen sodium phosphate, which decomposes the sodium salt, but does not remove the magnesium, and then extracting the mixture with ether. The product can be further purified by extracting the ethereal solution with *monohydrogen disodium phosphate*, which is sufficiently alkaline to extract the acidic chlorophyllin; some of the accompanying impurities remain in the ethereal solution. On adding the *dihydrogen monosodium phosphate* to the aqueous solution, the chlorophyllin is again set free from its sodium salt and can be extracted with ether. In this way a somewhat weakly acid product was obtained from several varieties of plants, which was non-crystalline in character and which invariably contained magnesium in quantities of from 2.3 to 3.5 per cent., which could be readily eliminated from the molecule by the action of weak acids.

Phæophytin and Phytol.—After having investigated the gentle action of alkalis on chlorophyll extracts attention was turned to the gentle action of acids, and it was found that the first

reaction which took place was the elimination of magnesium. A product was thereby obtained which, unlike chlorophyllin or alkachlorophyll, possessed no acidic characters, but was a neutral product, to which the name *phæophytin*¹ was assigned. This substance on hydrolysis with alkali yields the alcohol, which has been already mentioned, which possess a composition corresponding with the formula $C_{20}H_{40}O$, and to which the name phytol was assigned.

Phæophytin was prepared by treating the alcoholic extract of leaves with just sufficient oxalic acid; it separates out as a product somewhat insoluble in alcohol, and was freed from its admixtures by repeated solution in chloroform, and precipitation from this solution by means of alcohol. The product obtained in this way possesses neither marked acidic nor basic properties; it is a wax-like substance, which in solid form is almost black, and which gives rise to dark olive-brown fluorescent solution. The products obtained from various sources did not all have the same chemical composition; some specimens obtained from the same species of plants, but from different harvests, varied. The reason of this is due to the admixture of another closely allied substance which will be alluded to later. The phæophytin from different sources yields, furthermore, for the same reason, on treatment with alcoholic solutions of caustic alkalis varying quantities of phytol. Thus a sample of phæophytin from grass yielded 32 per cent. of its weight of phytol, whereas one from pines yielded only 14 per cent. The phæophytins from various sources yielded on hydrolysis with alcoholic potassium hydroxide or by strong acids varying quantities of mixtures of the different phytorhodins and phytochlorins.

The alcohol phytol, $C_{20}H_{40}O$, obtained by the hydrolysis of phæophytin, is a clear colourless oil which boils at a temperature of 145° under a pressure of $0.03-0.04$ mm. It does not distil without decomposition if the pressure be much higher, and it cannot, therefore, be fractionated in an ordinary vacuum. It can be readily oxidised by potassium permanganate and also chromium trioxide. The esterification constant was determined by the Menshutkin method; this constant was not far removed from that of allyl alcohol; its behaviour agreed generally with that of a primary

¹ With regard to the nomenclature, Willstatter proposes to adopt the termination “-phytin” for the products derived by treatment of chlorophyll with acids, and “-phyllin” for those containing magnesium and derived by the action of alkalis.

olefin alcohol. Several crystalline derivatives were obtained and also, by treatment with an acetic acid solution of hydriodic acid, and afterwards with zinc, a hydrocarbon phytene, $C_{20}H_{10}$.

Crystalline Chlorophyll.—As already mentioned, different preparations of phæophytin yielded varying quantities of phytol on treatment with alcoholic potassium hydroxide. This fact suggested at first that the phæophytins from different plants were not all identical. Further research revealed the fact, however, that in addition to the neutral chlorophyll which was the ester of phytol, another body existed in plants which did not yield phytol on hydrolysis. This product was the crystalline chlorophyll, which had been originally described by Borodin and later by Monteverde. The latter investigator distinguished between three classes of plants, in respect to chlorophyll yields, viz. those which yield on extraction with alcohol chiefly amorphous chlorophyll, those yielding a mixture of the two substances, and those which yield principally the crystalline product. Amongst this latter class occur several varieties of galeopsis, which was found the best adapted for the preparation of a crystalline chlorophyll. The dried leaves were mixed with chalk to neutralise the plant acids, then extracted with alcohol, and the alcoholic extract was then thrown into ether. The ether alcohol solution was washed with water several times to remove the alcohol, after shaking the solution in organic solvents with talc to get rid of resinous matter, which impedes the separation into two layers of the water and ether. On evaporation of the ethereal solution thus obtained the crystalline chlorophyll separated. When obtained by this method it was generally mixed with a colourless substance, from which it could be separated by re-solution in ether, in which the foreign body does not readily dissolve. The product obtained in this way crystallised in hexagonal or triangular plates of bluish-black colour, with no melting-point, and was readily soluble in alcohol, wood-spirit and acetone, but soluble only with some difficulty in ether; it was readily soluble also in chloroform, and easily soluble in hot methylal, from which it separated on cooling; this solvent was found to be a convenient one for the recrystallisation of the substance. Its composition agreed well with the formula $C_{36}H_{42}O_7N_4Mg$. On gentle treatment with alcoholic alkalis, crystalline chlorophyll yielded a chlorophyllin; by heating with methyl-alcoholic potassium hydroxide at $140-200^\circ$ it yielded

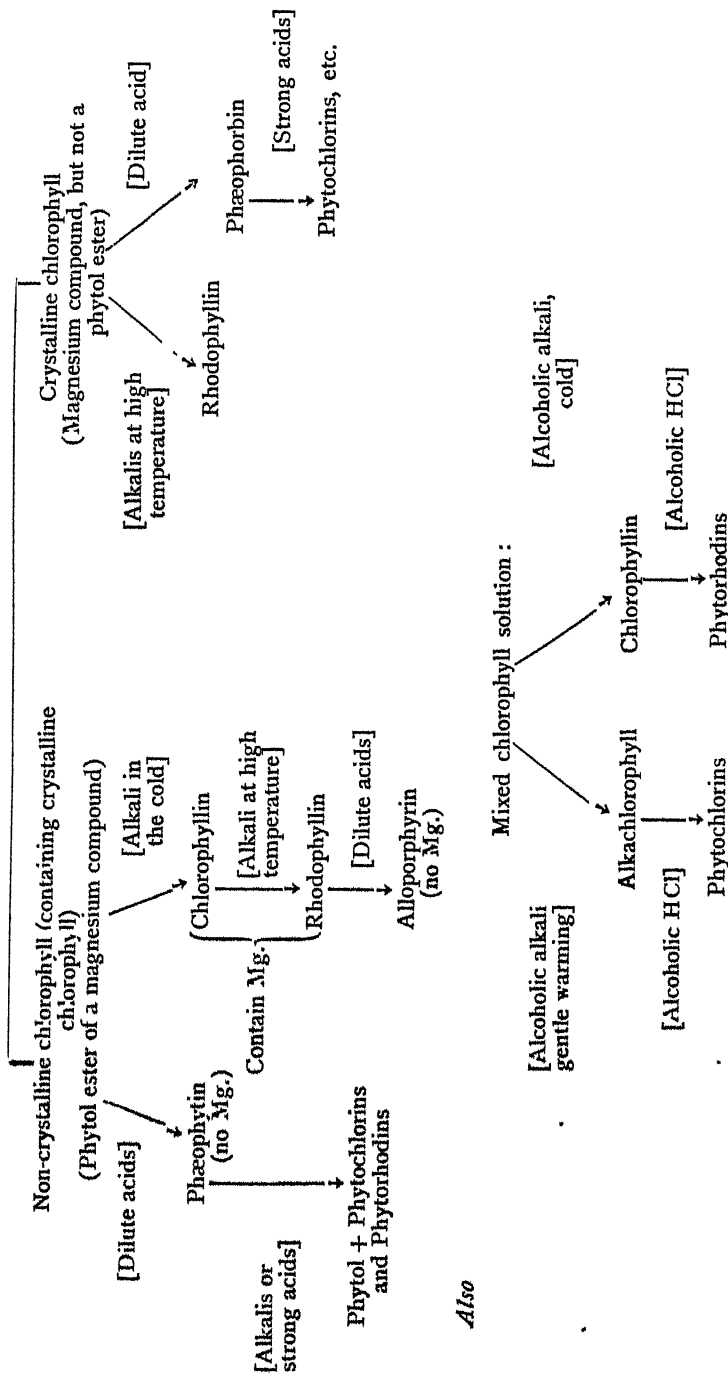
rhodophyllin (which will be described below). It could be freed from magnesium by treatment with oxalic acid, and yielded, not phæophytin, but a product from which no phytol was obtainable by alkaline hydrolysis, and which was designated *phæophorbin*. This substance reacts in the cold with neither acids nor bases, but is a neutral product. On warming with methyl-alcoholic potassium hydroxide, it yielded, however, phytochlorins and phytorhodins.

Rhodophyllin and Alloporphyrin.—The more vigorous action of alkalis on chlorophyllin was also investigated by Willstätter. By the action of alcoholic potassium hydroxide at 140–200° in an autoclave (with the reaction mixture in a silver beaker) a product still containing magnesium was obtained, the composition of which corresponded with the formula $C_{33}H_{34}O_4N_4Mg$. This product, which was called *rhodophyllin*, is of great interest, in that its chemical composition corresponds very closely with that of the blood-pigment hæmatin, to which Zaleski has assigned the formula $C_{34}H_{33}O_5N_4Fe$. The analysis will agree quite closely, however, with the formula $C_{33}H_{33}O_5N_4Fe$. As will be evident from considerations advanced below, the correspondence between the hæmatin and rhodophyllin can hardly be accidental.

Rhodophyllin crystallised in beautiful shining prisms, with a deep blue surface colour; it was readily soluble in alcohol, but with difficulty soluble in ether, and formed easily crystallisable salts. It was readily decomposed by acids, with elimination of magnesium. In this way a new product, designated *alloporphyrin*, was obtained, which possessed exactly the same chemical composition as a product obtained by Nencki and Zaleski by the reduction of phylloporphyrin, which they called *mesoporphyrin* and which differed from the latter only in its oxygen content (see below on "relation of chlorophyll to blood-pigment"). Alloporphyrin is insoluble even in hot alcohol, and in most other organic solvents. It is easily soluble, however, in pyridine. It is insoluble in very dilute hydrochloric acid, and in this respect differs from the isomeric mesoporphyrin, which is readily soluble. It forms an insoluble sodium salt, and in this respect also differs from its isomer. On treatment with acetic anhydride it yields an anhydride.

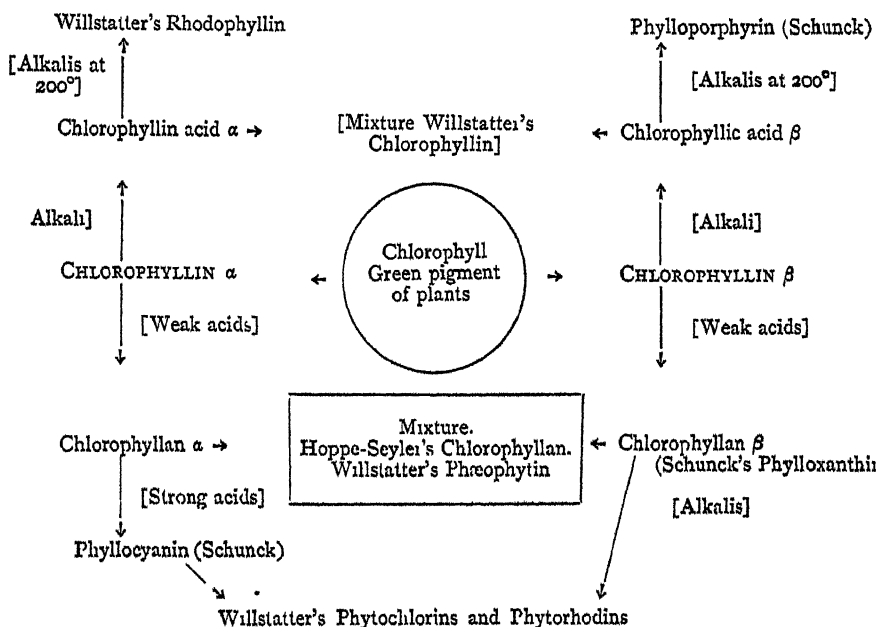
The above forms a short account of the various derivatives of the two chlorophylls (crystalline and non-crystalline), which have been described by Willstätter up to the time of writing. The genetic relationship of the various substances to one another can be schematically represented as follows :

GREEN PIGMENTS OF PLANTS.

*Also*

WORK OF TSWETT AND MORE RECENT WORK OF MARCHLEWSKI

Tswett has devised another method for investigating chlorophyll derivatives—the so-called method of adsorption analysis. Solutions of the pigment are made to pass through a filter of adsorbent material—calcium carbonate being most commonly used. The more readily adsorbed pigment will be retained in the upper layers of the filter, the less readily adsorbed in the lower layers. The layers are then divided after filtration, the pigment re-extracted from each, and then submitted to spectroscopic examination. By this method, Tswett claims to have separated two main constituents, *chlorophyllin a* and *chlorophyllin β* from chlorophyll, and has studied the action of acids and alkalis on these substances, and expresses the relationship of the various chlorophyll derivatives to one another according to the following scheme :



It must be remarked that this scheme depends chiefly on the adsorption analysis method, and spectroscopic examination. The chemical evidence upon which it rests appears at the present moment to be somewhat slender.

Marchlewski has recently examined the phaeophytin as prepared by Willstätter's method. He has suggested that for this

product the old name of chlorophyllan be retained. He finds, using the acid fractionation method, that is probably a mixture of a pure chlorophyllan proper, with more basic substances, of probably a phyllocyanin, or phytochlorin type.

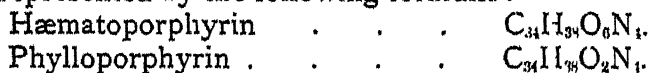
He has also shown quite recently that phyllotaonin, on warming, is readily converted into another derivative, viz. '*allophyllo-taonin*', which is a weaker base than phyllotaonin, and can be re-converted into this substance on treatment with weak alkali. On treatment with alcoholic hydrochloric acid (20 per cent.) it can be converted into substances of the phytorhodin type.

THE CHEMICAL RELATIONSHIP BETWEEN CHLOROPHYLL AND THE BLOOD-PIGMENTS

As already mentioned, Hoppe-Seyler noticed a similarity between the product obtained by him by treating chlorophyllan with caustic alkalis at high temperature, and the pigment previously obtained from hæmatin; for this reason he gave to the product the name of phylloporphyrin to denote its apparent relationship to the blood-pigment hæmatoporphyrin.

Subsequently, Schunck and Marchlewski succeeded in preparing phylloporphyrin in a condition of much greater purity than it had hitherto been obtained, and made a comparison between its absorption spectrum and that of hæmatoporphyrin. They noticed an astonishing similarity, which demonstrated beyond doubt the intimate chemical relationship between the two bodies.

In the meantime, hæmatin and its derivative hæmatoporphyrin had formed the subject of very careful investigation by Küster on the one hand, and by Nencki and Zaleski on the other. It was found as the result of their investigations that hæmatoporphyrin and phylloporphyrin differed from one another only in their oxygen content. Adopting the formulæ most in accordance with the careful analyses of Zaleski, the two substances can be best represented by the following formulæ:



Nencki and Zaleski then attempted to convert the one body into the other by reduction of hæmatoporphyrin with hydriodic acid of phosphorus. Instead of obtaining phylloporphyrin, they produced an intermediate substance, which yielded a crystalline hydrochloride, and which had a chemical composition inter-

mediate between hæmatoporphyrin and phylloporphyrin. To this they assigned the name *mesoporphyrin*. Accepting the formulæ of the two former bodies as given above, its composition should be represented by the formula $C_{31}H_{38}O_4N_4$.

A further relationship between the blood-pigments and chlorophyll was subsequently discovered by Willstätter, when by the action of acids on rhodophyllin he obtained the substance alloporphyrin, which has almost exactly the same empirical composition as mesoporphyrin, although it differs from it in certain particulars.¹

Willstätter also demonstrated an intimate relationship in the chemical composition between the magnesium derivative rhodophyllin and the iron blood-derivative hæmin.

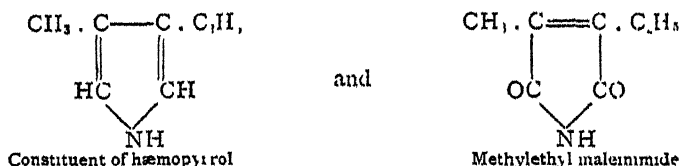
In addition to mesoporphyrin, Zaleski and Nencki obtained by the reduction of hæmatoporphyrin, a volatile product which yielded a crystalline picrate and an insoluble mercury compound, and which gave with sulphuric acid and pine splinters the colour reaction which is characteristic of pyrrol derivatives. To this they assigned the formula $C_8H_{13}N$ and designated it *hæmopyrrol*. On standing in air it oxidised to a red substance, which changed to yellow on addition of alkali, and to a pink colour with a beautiful green fluorescence on the addition of an ammoniacal solution of a zinc salt. These reactions are characteristic of the urinary pigment urobilin, the cause of urobilinaria after blood extravasations.

At this stage of the investigations, Nencki and Marchlewski united in their work, and showed that phyllocyanin, the mother-substance of phylloporphyrin, also yields, on reduction with hydriodic acid and phosphonium iodide, the substance hæmopyrrol. These investigations established without doubt the intimate chemical relationship between chlorophyll and the blood-pigments.

The constitution of hæmopyrrol has formed the subject of numerous investigations by Küster, who has shown that it is apparently a mixture of two substances, one of which is more basic than the other. The more acid derivative yields on oxidation an imide which is identical with methylethyl maleinimide. The two substances contained in hæmopyrrol are probably

¹ The formula assigned to alloporphyrin in Willstätter's paper is $C_{30}H_{36}O_4N_4$. With bodies of this high molecular weight, it is impossible, of course, to obtain the correct formula from the analysis results alone.

3 methyl, 4 ethylpyrrol, and 3 methyl, 4 ethylpyrroline. The relationship between the pyrrol and the imide may be represented by the formula :



Groups of this nature are probably important constituents of both the chlorophyll and hæmin molecules. The constitutions of such groups cannot be regarded, however, as definitely settled.

OTHER PIGMENTS WHICH ACCOMPANY CHLOROPHYLL IN PLANTS

It has been already mentioned that one of the great difficulties in the chlorophyll investigations related to the separation of the green from the other pigments. Various methods were devised, notably by Kraus and by Sorby, for the separation of these substances. Kraus showed that by shaking an alcoholic extract of green plants with benzene or benzine, and then separating into two layers by means of water, the spirit solution became yellow-coloured, whereas the hydrocarbon retained the green pigment. Sorby employed a similar method, using carbon disulphide instead of a hydrocarbon. In this way it was supposed that chlorophyll could be separated from the yellow pigment.

The isolation of such a pigment in crystalline form which had been repeatedly noticed by botanists in microscopic preparations was the subject of numerous researches. The chief of the earlier investigations are due to Arnaud, who thought that the crystalline pigment which he isolated was probably identical with the pigment of carrots. This supposition was subsequently confirmed by Monteverde; it was consequently called *carotin*.

Recently this substance has been the subject of a more detailed investigation by Willstätter, who has assigned to it the formula $\text{C}_{40}\text{H}_{56}$. He obtained it from green leaves by extraction with petroleum ether, freeing the extract from the green pigments by washing with alkali; the carotin itself was purified by fractional precipitation of its solution in carbon disulphide with alcohol.

In addition to carotin, Willstätter also succeeded in isolating another pigment, *Xanthophyll*, with composition corresponding with the formula $C_{40}H_{56}O$. It is closely related in composition, therefore, to carotin. It was obtained from the mother-liquors of a chlorophyllin preparation, by mixing the alkaline alcoholic extract of leaves after addition of alkali and separation of the potassium salt, with ether, washing out alkali and alcohol from the ethereal extract, and finally precipitating the pigment from this by light petroleum. The precipitate was purified by dissolving in hot acetone, and adding wood-spirit to the solution: from this mixture the xanthophyll separated.

Both carotin and xanthophyll are unsaturated, and the opinion is expressed that carotin may act as an oxygen carrier, and be converted into xanthophyll, which may, under suitable conditions, again yield up the oxygen. The general properties of the two substances are summarised in the accompanying table:

	CAROTIN.	XANTHOPHYLL.
Formula.	$C_{40}H_{56}$.	$C_{40}H_{56}O_2$.
Appearance	Copper-coloured leaves	Pleochromatic dark-brown plates
Crystalline properties	Nearly quadratic	Trapezium-form, with tendency to twin-formation
Solubility in light petroleum	Appreciably soluble	Insoluble
Solubility in alcohol	Almost insoluble in cold. With great difficulty in hot	With difficulty in cold. Fairly easily in hot
Solubility in acetone	Soluble with great difficulty	Easily soluble
Solubility in cold carbon disulphide	Easily soluble	Soluble with difficulty
Colour in transmitted light	Red	Yellow to orange
Melting-point	167.5-168	172

GENERAL CONCLUSIONS

Chlorophyll is a magnesium derivative, from which the metal is readily eliminated by means of acids. It is assumed by Willstätter that magnesium plays an important part in the synthetic functions of the green pigment, acting in a manner similar to that in which it acts in Grignard's reaction; the iron in the allied pigments of blood, on the other hand, is assumed to play an important part in oxidation. The magnesium is removed with very great difficulty by alkalis.

The green pigment consists in most plants apparently of a mixture of magnesium derivatives, a crystalline chlorophyll, and another chlorophyll or mixture of chlorophylls, the latter of which are esters of the unsaturated alcohol phytol.

According to Willstätter, the chlorophylls yield, on treatment with alkalis and acids, complex mixtures of bodies of varying basicity known as the phytorhodins and phytochlorins. Other derivatives, obtained by the double action of acids and alkalis, have been described by Schunck and Marchlewski, which are for the most part probably mixtures, and which cannot up to the present be identified with certainty with those described by Willstätter.

There is a considerable amount of evidence which tends to show that an intimate chemical relationship exists between the green pigments of plants and the red pigments of blood.

Owing to the large number of products described there is a considerable difficulty in following the literature of chlorophyll chemistry. An attempt has been made above to trace historically the discovery of the more important of these, and it is hoped that the summary given will prove of use to those who desire to follow the future discoveries in this important branch of biological chemistry.

Note.—For the sake of uniformity, the names of substances such as photochlorin and phytorhodin have been spelt without a terminal *c*, although many of them are of distinct basic character.

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VERTEBRATE PALÆONTOLOGY IN 1908

By R. LYDEKKER

So far as it has at present come to hand the work accomplished in the domain of vertebrate palæontology during 1908 is both voluminous and important, comprising several memoirs of more than average interest and value, in which our knowledge of local faunistic groups is consolidated and brought up to date, with, in most instances, descriptions of new species. At no period was work of the latter nature of greater importance than it is at present, when isolated papers are published in so many parts of the world that it is a matter of the greatest difficulty for the student to be assured that he has made himself acquainted with all the recent literature bearing on his particular subject. With these few preliminary remarks, the more important papers (inclusive of a few published in 1907) that have come under the writer's notice during the year may be passed in review.

Both as regards the nature of its subject and its importance and interest from a taxonomic standpoint, the first place is claimed by Dr. H. F. Standing's memoir on recently discovered fossil Primates from Madagascar, published in the eighteenth volume of the *Transactions of the Zoological Society of London*, with an appendix by Professor Elliot Smith on the brain of the extinct Malagasy lemurs and the affinities of the sifaka and indri group. The memoir commences with an account of the marsh near Ampasambazimba, which has yielded the remains of these marvellous lemuroids, as represented by species of the size of the larger existing forms to others rivalling the chimpanzee or gorilla in stature. The region is essentially volcanic, and the ancient lake which formed the tomb of the lemur-like creatures appears to have been mainly held up by a dam of lava; and there is evidence that in former times a very large extent of the country was under water, while the land-surface was heavily forested. Pottery, apparently of Chinese manufacture, has

been found in the upper layers of the marsh; and, since there are traditions in the island of the former existence of an extinct hippopotamus and of a creature which may well have been the *æpyornis* (probably the "roc" of the Arab voyagers), the author is of opinion that the most recent of the extinct lemuroid remains are not more than five centuries old. This is equivalent to saying that in the reign of Henry IV., Madagascar was the home of the roc, a hippopotamus, and a host of giant lemurs.

Dr. Standing's classification of the lemurs differs considerably from that of previous writers, the whole group being divided into the families *Indrisidæ*, *Lemuridæ*, and *Tarsiidæ*. In the former are included not only the extinct *Archæolemurinæ* and the modern *Indrisinæ* (sifakas), but likewise the aye-aye, the sole representative of the *Chiromyinaæ*, which is regarded, and doubtless rightly, merely as a specialised form. In the *Lemuridæ* are comprised the extinct *Megaladapinæ* together with the modern African *Galaginæ* and the Asiatic and West African *Lorisinæ*, the *Tarsiidæ* being, of course, represented merely by the tarsier. It may be of interest to contrast this classification with that of Max Weber, in which the group is divided into *Tarsiidæ*, *Lemuridæ*, and *Nycticebidæ*; of which the second family includes the *Lemurinæ*, *Indrisinæ*, and *Chiromyinaæ*, while the third comprises the *Galaginæ* and *Lorisinæ*. The characters presented by the tympanic ring, on which Prof. Max. Weber bases his classification, are ignored by Dr. Standing.

The most important part of the author's investigations is that connected with the relationship of the lemuroids to the higher Primates—a matter in regard to which there has been very considerable diversity of opinion among naturalists. In the first place Dr. Standing definitely accepts the view that Africa and Madagascar formed the eastern portion of a great continent, extending across the Atlantic to South America, and that this connection persisted till well into the Tertiary epoch; while he is further of opinion that the connection between Madagascar and Africa persisted still later, possibly to the Miocene. On this continent lived, during the Eocene, a primate stock with most of the distinctive simian characters; which by the Oligocene had developed into *Cebidæ* on the American, and by the

Miocene into an Old World type of monkey on the African side. When this stock became insulated in Madagascar it had acquired characteristics of both these groups, the resemblance being closest with the former. Specialisation, and subsequently degradation, characterised the Malagasy stock. Finally, it is concluded that the separation of Lemuroidea from Anthropeidea is no longer justifiable, the author adding that in his opinion there is no close relationship between the recent representatives of the former and such ancient northern types as *Adapis* and *Necrolenur*.

Here may be appropriately noticed a paper by Dr. Max Schlosser, published in the *Festband* (1907) of the *Neues Jahrbuch für Mineralogie*, on the osteology and systematic position of the Oligocene genus *Necrolenur*, with special reference to the phylogeny of the Primates in general.

In this paper the author divides the Primates into the three groups, Mesodonta, Lemuroidea, and Anthropeidea. The first is sub-divided into three sections, viz.: Pseudolemuroidini, with the families *Hyopsodontidae*, *Notharctidae*, and *Adapidae*; (2) Palæopithecini, with the families *Anaptomorphidae* and *Tarsiidae*; and (3) Mixodectini, with *Oldobotidae* and *Microsyopidae*. The *Hyopsodontidae* may be the ancestors of the Anthropeidea, or, in any case, the *Cercopithecidae* may be derived from this family. The *Notharctidae* are regarded as ancestral to the *Simiidae*. The *Adapidae*, on the other hand, are considered to have given origin to the Malagasy *Megaladapinae*, and perhaps also to the *Lemurinae* and *Indrisinae*. The whole group is regarded as of northern origin, and not to have reached Africa till the Middle Miocene, and Patagonia till the Upper Miocene.

Dr. Schlosser's views, it will be observed, differ *in toto* from those of Dr. Standing. Personally I am inclined to accept, with some modifications, the former. On this view only the ancestors of the lemuroids, and perhaps of the *Cebidae*, obtained an entrance into Africa; but probably at a period considerably earlier than supposed by the German palæontologist.

Before leaving the Primates it should be mentioned that considerable interest attaches to the discovery in the Norfolk "forest-bed" of the lower extremity of the humerus of a monkey, probably referable to the genus *Macacus*. In describing this specimen in the *Geological Magazine* for October,

Mr. M. A. C. Hinton takes occasion to discuss the objections which have been taken to the reputed place of origin of the type molar of *Macacus pliocenicus*, and concludes that there is no reason to doubt its being really from the Pleistocene clay of Grays, Essex.

The work on fossil Carnivora appears to have been comparatively small; but attention may be directed to a paper by Prof. A. Martelli, published in the *Boll. Soc. Geol. Ital.* for 1906, in which the author describes two new mustelines from the Tuscan Pliocene, under the generic title of *Proputorius*, and likewise a new cat. Reference may also be made to a paper by Madame Pavlow, published at Odessa (*Mém. Soc. Nat. Nouv. Russ.*, 1908), on the fossil carnivora of the Chersonese and Bessarabia, which includes the description of a new species of the Pliocene civet-like genus *Ictitherium*. Those interested in the origin of our domesticated dogs should refer to a paper by Dr. T. Studer, published in *Mitt. Naturfor. Ges.*, 1907, p. 155, on the skull of a dog from a prehistoric dwelling near Karlstein, Amtsgericht Reichenhall, in which the whole question is discussed in considerable detail.

In describing this skull, Dr. Studer takes the opportunity of reviewing the state of our knowledge of prehistoric dogs generally. In the Palæolithic epoch we have *Canis poutjatini*, an animal of the size of a German sheepdog, with all the general characters of *C. familiaris*, but showing affinity with the dingo of Australia and *C. tenggerianus* of Java. This dog probably lived with Palæolithic man in a half-wild condition, and, by crossing with the wolf, seems to have given rise to a breed like the "laiki" of Siberia, this being represented by *C. inostranzewi* of Lake Ladoga and the Pfahlbauten of Lake Neuenburg, while by a cross with a flat-headed wolf the Neolithic *C. leineri*, the ancestral form of the modern deerhounds, arose. In another line we have from *C. poutjatini* the sheepdogs, and in yet another the hound-group, whose earliest representatives are *C. matris-optimæ* and *C. intermedius* of the Bronze age. Perhaps by further crossing with the wolf or with *C. inostranzewi* was produced the small *C. familiaris palustris* of the Pfahlbauten. Crossing of the larger breeds, aided perhaps by intermixture with high-skulled wolves, gave rise to the boarhound group, to which the Karlstein skull pertains, this group not making its appearance until the Glacial period. The

group seems to have been characteristic of the Alpine region, where it is still represented by the St. Bernard.

In the Ungulate order a large amount of work has been accomplished during the year, one of the most important contributions to our knowledge of this group being a paper by Dr. W. D. Matthew, published in vol. xxiv. of the *Bulletin of the American Museum of Natural History*, on the osteology and phylogeny of the American *Cervidae*. In this communication the author suggests that the primitive Oligocene ruminant *Leptomeryx* (hitherto generally regarded as a traguline) is the ancestor of the exclusively American deer; the annectant form being *Blastomeryx*—a larger animal with long upper tusks like those of a musk-deer—from which sprang the existing brockets (*Mazama*). That *Leptomeryx* is not the ancestor of the deer as a whole is demonstrated by the occurrence of deer-like ruminants of a higher type in the European Oligocene. Deer are evidently a Holarctic type, and the American forms are supposed to have originated independently on the western side of that realm. The difficulty in accepting this view seems to me to be the general similarity of American to the Old World deer; if they had independent origins from antler-less ruminants they should be much more unlike. No reference is made by the author to the opinion that the Tertiary European genus *Anogloche* is near akin to the American deer.

In connection with the foregoing, reference may be made to a paper by Prof. Simonelli (*Mem. Ac. Sci. Bologna*, ser. 6, vol. iv., 1907) on the Pleistocene mammals of Crete, in which a new species of the aforesaid *Anogloche* is described.

Explorations in Alaska, as described by Mr. Gilmore in the *Miscellaneous Collections of the Smithsonian Institution*, have furnished new information with regard to the bison and other large Pleistocene mammals of that area; while from the Yukon Pleistocene, Mr. J. W. Gidley (*Proc. U. S. Nat. Mus.*, vol. xxxiv. p. 681) describes a new musk-ox. In the same communication this gentleman likewise named a species of the allied genus *Boötherium*, on the evidence of a skull from the superficial deposits of Michigan. Bison and oxen remains from Siebengebirge form the subject of a paper by Dr. O. Phelps in the *Verhandlungen* of the local society (vol. lvi., 1907). A new species of the extinct bovine genus *Symbos* from a fissure in Arkansas is described by Mr. B. Brown in a paper quoted later. In the camel group

Mr. O. Peterson gives an elaborate account of the osteology of the remarkable Tertiary American genus *Stenomylus*, as exemplified by *St. gracilis*. This memoir appeared in vol. xiv. of the *Annals of the Carnegie Museum*.

The perissodactylate section of Ungulates has received during the year a specially large amount of attention at the hands of palæontologists in America. Mr. H. J. Cook (*Amer. Nat.*, vol. xlii. p. 543) has, for instance, described a new hornless rhinoceros from the Nebraska Miocene; while in the *Annals of the Carnegie Institute* Mr. E. Douglas has brought to notice other members of the same group, as well as a number of ancestral forms of the horse phylum from the Miocene and Oligocene formations of Dakota and Montana. Mr. Walter Granger has also been working on the group last mentioned, and has published in the *Bulletin of the American Museum* (vol. xxiv. p. 221) an important revision of the Eocene species; while to the August issue of the *American Journal of Science* (vol. xxvi. pp. 51 and 163) Mr. F. W. Loomis has contributed a general account of the Miocene *Rhinocerotidæ*, and the description of a new species of *Parahippus* from the Nebraska Miocene. In Europe the fossil rhinoceroses of Samos form the subject of a paper by Mr. M. Weber, published at Stuttgart in the *Centralblatt für Mineralogie* (1907, p. 29).

Turning to the subungulate groups, special interest attaches to a paper, published in vol. xlvii. of the *Proceedings of the American Philosophical Society*, by Mr. W. J. Sinclair on the affinities of *Hegetotherium* and *Protypotherium*, the Mid-Tertiary Patagonian representatives of the later *Typotherium*. Attempts have often been made to show that these remarkable Ungulates were related either to the rodents or to the hyraxes. That the presumed relationship with the former group is a case of parallel development is, however, practically certain; and the author shows that no better case can be made out for the presumed hyrax-affinity. In the Hyracoidia, for instance, the carpus is of the linear type, with a separate centrale, while there is also a remarkable step on the inner side of the astragalus for the articulation of the tibial malleolus. In the Typotheria (which are regarded as a sub-group of Toxodontia), on the other hand, the carpus is arranged on the interlocking plan, there is no step to the astragalus, and there are certain features in the skull and dentition foreign to the hyraxes. This group is divided into

the families *Typotheriidae*, *Interatheriidae*, and *Hegetotheriidae*. A considerable amount of work has likewise been devoted to the elephant-group, on both sides of the Atlantic. In *Nature* (vol. lxxviii. p. 443) Mr. Lucas has discussed the height of the mammoth and its relatives, pointing out that, while the former seldom exceeded nine feet, the allied *Elephas columbi* was one of the biggest of all elephants, towering to thirteen feet or even more. A contemporary note on a mastodon skeleton discovered near Chester, U.S.A., in 1817, is published by Mr. E. V. Hovey in the April issue of the *Annals of the New York Academy*; while Professor Capellini has described (*Mem. Ac. Sci. Bologna*, ser. 6, vol. iv., 1907) the collection of remains of the same genus in the museum at Bologna. Some of the large dinotheretech, for which Professor Stefanescu has suggested the name of *Dnotherium gigantissimum*, are discussed by that palæontologist in the *Comptes Rendus* of the tenth Geological Congress, held in Mexico in 1906. Of more general interest is an account, by Dr. R. S. Lull, of the evolution of the elephant-group, originally published in the *American Journal of Science*, but subsequently issued as a guide-book to the Peabody Museum of Natural History. More important than all is a memoir by Dr. C. W. Andrews, published in vol. cxcix. of the *Philosophical Transactions*, on the skull and milk-dentition of the Egyptian Eocene genus *Palæomastodon*, with observations on the mode of replacement of the cheek-teeth in the Proboscideæ generally. Attention is specially directed to the gradual elimination of deciduous premolars (of which three pairs were developed in the earlier forms); while another feature of more than ordinary interest is the gradual development of the great supinator ridge of the humerus of modern elephants, of which no trace is visible in the pigmy Eocene *Merritherium*.

In this place reference may be appropriately made to a paper by Professor H. F. Osborn, published in the *Bulletin of the American Museum of Natural History* (vol. xxiv. p. 265), in which the author describes several apparently new types of Eocene mammals on the evidence of remains obtained during the recent American expedition to the Fayum. Unfortunately, these remains are so fragmentary that little or no information can be gleaned from them in several instances as to the systematic position of the creatures to which they pertained. For two of these problematical forms the new generic names *Apidium* and

Ptolemaia have been respectively proposed, the latter being made the type of a family of uncertain position. Whether the true affinities of these fossils will ever be ascertained remains to be seen. More satisfactory is the determination of two new genera, *Phiomys* and *Metaphiomys*, of the Eocene rodent family *Eomyidae*. These appear to be the only new fossil rodents (other than Pleistocene) described during the year. *Apidium phiomensis* was evidently a small frugivorous or omnivorous creature, with partly cuspidate cheek-teeth; but at present its affinities are indeterminable.

Beaver-skulls from the British Pleistocene are discussed by Dr. Forsyth Major in the Zoological Society's *Proceedings*; while others from Continental deposits form the subject of an article by Dr. G. Hagmann in *Mitt. Geol. Landesanst. Elsass-Lothringen*, vol. vi., p. 369.

The above mention of a faunistic paper naturally leads on to others, among which reference may be made to one by Mr. B. Brown, published in the *Memoirs of the American Museum* (vol. ix. p. 157), containing descriptions of twenty-two new species and two new genera of mammals from an ossiferous fissure in Arkansas; none of which are, however, of very special interest. A translation of part of Professor Depéret's valuable paper on the Tertiary migrations of mammals has been published in the *American Naturalist*. Reference may be made in this place to Mr. Cushman's list of the type specimens preserved in the Boston (U.S.A.) Natural History Society, which appeared during the year in vol. xxxiii. of the *Proceedings* of that body.

Three papers on fossil American cetaceans have been published by Mr. F. W. True. In the first of these, which appeared in the February issue of the *Proceedings of the Philadelphia Academy*, the author discusses the fossil Cetacean beak from Charles County on which Cope founded the genus and species *Rhabdosteus latiradix*, together with certain other fragmentary beaks and teeth which have been assigned to the same form. In Mr. True's opinion it is probable that, while the teeth belong to the widely-spread genus *Schizodelphis*, the type beak is generically distinct. Of the other two beaks, one apparently indicates a dolphin allied to the Amazonian *Inia*, while the third may be provisionally assigned to the extinct genus *Priscodelphinus*. In the second communication (*Smithsonian*

Miscell. Coll., vol. ii. p. 449) the author discusses the occurrence in the United States of remains of dolphins referable to *Schizodelphis*, and likewise the affinities of Case's *Priscodelphinus crassirostris*. Lastly, we have a paper in the *Bulletin of the Harvard Museum* (vol. lii. p. 65) on *Dorudon serratus*, an extinct cetacean described many years ago by Gibbs, of which the affinities are doubtful. To the *Geological Magazine* for May Dr. C. W. Andrews contributed an account of a restored skull of *Progeniulodon*, one of the links connecting the extinct creodont carnivora with the typical cetaceans. Reference may here be made to a revised classification of the cetacea, largely based on palæontological evidence, by Mr. True, published in the *Proceedings of the American Phil. Soc.* for 1908.

Considerable morphological importance attaches to a paper by Dr. F. Ameghino, published in vol. xvii. of the *Anales* of the Buenos Aires Museum, on the scapular arch of edentates and monotremes. It may be recalled that in the Zoological Society's *Proceedings* for 1893 the present writer proposed a revision of the names in general use for certain portions of the pectoral girdle, suggesting that the avian and reptilian so-called coracoid ought to be termed metacoracoid, while the name coracoid should be restricted to the process bearing that name in the higher mammals, and to the bone generally termed epicoracoid in dicynodonts and monotremes. As the result of a comparison of a number of both extinct and living types, Dr. Ameghino is convinced that this nomenclature is the true one, and ought in consequence to be generally adopted.

A second paper by Dr. Ameghino in the same volume of the *Anales* of the Buenos Aires Museum is devoted to the alleged occurrence of remains of armadillos in the Oligocene of France and Germany. In view of the opinions which have been advanced as to the reptilian nature of these remains, which consist mainly of the plates of the dermal head-shield, the author states that he can no longer definitely assert that such plates indicate the occurrence of armadillos in the European Oligocene. At the same time, he refers to a figure of the microscopic structure of one of these plates, published by Filhol, which accords very closely with similar figures of recent armadillo-plates, and differs markedly from that presented by the plates of certain lizards. To settle the question, it is urged that sections of the plates of the so-called *Necrodusypus* should

be compared with those of the dermal armour of the approximately contemporaneous lizard *Placosaurus*.

One of the most important pieces of palæontological work accomplished during the year relates to the extinct birds of the Oligocene phosphoritic deposits of Central France. These remains have been described in great detail by Dr. Gaillard—who has had the opportunity of inspecting nearly all the more important specimens—in a memoir published by the medical faculty of the University of Lyons. This task was one of very considerable difficulty owing to the fact that the bones are very generally broken and always disassociated. The special interest of the Quercy bird-fauna, as now revealed to us, is connected with geographical distribution; the remains indicating a most remarkable mixture of what are now Old World, and especially African, types on the one hand, with those at present characteristic of Central and South America on the other. We have, for instance, extinct generic types of secretary-birds associated with species near akin to modern condors and trogons. As a somewhat similar feature is likewise characteristic of the early Tertiary bird-faunas of other parts of France, we seem to have additional and forcible evidence in favour of the view that during Secondary and early Tertiary times Africa was connected with South America by a great belt of land across the Atlantic. It is the many different lines of evidence by which this theory is supported that tend to raise it to a practical certainty.

The year has witnessed several important memoirs on fossil reptiles. In the first place, Professor von Huene has continued his valuable account of the Triassic dinosaurs of Europe and Asia (with special reference to the Asiatic types) in Koken's *Geol. und Pal. Abhandlungen*, six parts of text having appeared during 1908. Of equal importance is a monograph of the Ceratopsia, or horned dinosaurs of the Cretaceous, issued as monograph No. xlix. of the United States Geological Survey. The osteological portion of this memoir was written by the late Mr. J. B. Hatcher, whose untimely death rendered necessary its completion by some other palæontologist. Dr. R. S. Lull accordingly undertook the task of editing and completing the bulky volume. According to Mr. Hatcher, the horned dinosaurs are probably an exclusively American group, none of the European dinosaurs placed therein having any claim to such a position. In the case of Wealden bone, described as a

ceratopsian horn, the opinion is expressed that it is a weathered ungual phalange of a member of the sauropod group.

The earliest known Ceratopsia occur in the Judith River beds, but of the ancestors of the latter we have no knowledge, possibly for the reason that they were inhabitants of dry land, instead of, like their successors, of swamps. The members of the group living at the Laramie epoch exhibit advance over their predecessors in the matter of size, the preponderance of the supraorbital horns over the single nasal one, the fuller development of the flange-like neck-shield of the skull, and the perfection of a peculiar type of dentition.

That the horned dinosaurs were herbivorous is manifest; and it is suggested that while the edentulous, horny beak served for cropping succulent leaves and shoots, the teeth in the sides of the jaws chopped the food into short fragments. Swamps were apparently the home of these dinosaurs; and this may negative the idea that they were exterminated by predaceous mammals, since the latter have been supposed to be arboreal. If, however, mammals are derived from theriodont reptiles, the theory that the early forms were arboreal requires reconsideration.

In connection with this group it should be added that to the May number of the *American Journal of Science* Dr. Lull contributed a paper on the head-muscles of dinosaurs, with special relation to the origin of the neck-shield in the horned group. Such evidence as can be obtained with regard to the cranial musculature is gleaned from the form and proportions of the skull, coupled with, in some cases, the marks of the attachments of the muscles themselves. From the analogy of chamæleons, which, although insectivorous, masticate their food, it is inferred that the horned dinosaurs had powerful temporal and feeble pterygoid muscles, thereby differing from crocodiles, in which, owing to the absence of mastication, the conditions in these respects are reversed. It is pointed out that the neck-shield of the Ceratopsia presents an analogy to the casque of the chamæleon-skull, both structures being a backward extension of the parietal segment to afford space for the temporal muscles. Owen's chamæleon, from the Cameroons, presents a "mimicry" of the *Triceratops* type in carrying three horns situated much as in the dinosaur, although the horns are of dermal origin, and devoid of bony cores.

In the February number of the *American Journal of Science* (vol. xxv. p. 113), Messrs. Huene and Lull point out that the Triassic American reptile *Hallopas victor*, regarded by its original describer, O. C. Marsh, as a theropod dinosaur, in the structure of the pelvis is more like an orthopod dinosaur, although in the form of the pubis, the calcaneum, the extreme thickness of the astragalus, the contour of the scapula, and the height of the ilium, the skeleton differs from all known members of that group. In the opinion of the authors (the grounds of which are promised in a later memoir), the genus appears to be most nearly related to *Aëtosaurus* and its allies.

A remarkable new type of armoured dinosaurs, the *Ankloyosauridæ* (preferably *Anchylosauridæ*) is typified by *Ankylosaurus magniventris*, from the Upper Cretaceous of Montana, described by Mr. Barnum Brown in the *Bulletin of the American Museum of Natural History* (vol. xxiv. p. 187). This reptile is related to the Jurassic *Stegosaurus*, but is specially characterised by its plated and sculptured skull; the large, flat, or low-ridged armour-plates of the body, some of which are united to form a continuous shield; the short-spined vertebræ, with their parapophyses never rising above the level of the centra; and the fusion of the hinder ribs with the vertebræ (whence apparently the generic designation). The family characters are summarised by the describer as follows: Skull massive, short, and broadly triangular, with dermal plates solidly coössified to cartilage-bones; teeth stegosaurioid, with ovately lanceolate, flattened crowns and crenulated edges; vertebræ amphiplatyan, solid throughout; neck short; backbone stiff; body broad, covered with heavy armour-plating arranged in rows. *Palæoscincus* and *Troödon*, from the Judith River Cretaceous, probably belong to the same family, as does also a dinosaur from the corresponding strata of Canada described under the preoccupied name of *Stereoccephalus*.

Turning to our own country, considerable interest attaches to the description by Dr. Smith Woodward (*Ann. Mag. Nat. Hist.*, ser. 8, vol. i. p. 259) of a tibia of the typical dinosaur, *Megalosaurus*, from the Lower Lias of Wilmcote, near Stratford-on-Avon, Warwickshire. The special interest attaching to this specimen is due to the fact that *Megalosaurus* had previously been known from the Lias solely by a single tooth in the British Museum from the lower division of that formation at Lyme

Regis. As the Liassic species is almost certainly distinct from those of the superior formations, it may be appropriately designated *Megalosaurus woodwardi*, with the tooth from Lyme as the type. Dr. Woodward has also shown, in the *Geological Magazine* for May, that the limb-bones from the Greensand of Hythe, described by Owen as *Dinodocus mackesoni* and regarded as sauropterygian, really indicate a gigantic sauropod dinosaur.

Evidence is gradually accumulating to show that during the middle of the Secondary epoch the dinosaurian reptiles ranged over the greater part of the world. They occur, for instance (mostly as isolated bones), all over temperate Europe, in both North and South America, in Madagascar, and now they have turned up in East Africa. Dr. E. Fraas, of Stuttgart, has recently published (*Mitteilungen Nat.-Kabinett, Stuttgart*, No. 61, 1908) an account of a large portion of the skeleton of a monster of this kind which he brought back from German East Africa. Some of the bones appear to have been lying on the ground, but others were dug up at a short distance below the surface. They comprise a number of the vertebræ, the haunch-bones, and most of the bones of the hind-limbs, inclusive of the huge claw-bones of the feet. As the thigh-bone measures close upon a yard and a half in length the African monster (for which the name *Gigantosaurus*, despite its having been previously used for a dinosaurian bone from Cambridgeshire, has been proposed) must have rivalled *Diplodocus* in point of size. The memoir is illustrated by the reproduction of a photograph showing the bones as they lay on the ground in front of the party of natives by whom they were disinterred; and there seems little doubt that the whole of the skeleton must have been buried in the same spot, together with one of a second species. And where one or two skeletons have been found others ought to occur in the neighbourhood. Dr. Fraas is on his way to German East Africa for the purpose of collecting other specimens.

Passing from dinosaurs to crocodiles, we find that Mr. L. M. Lambe has contributed a paper to the first volume of the third series of the *Transactions of the Royal Society of Canada* on the remains of a new type of crocodile from the Cretaceous formation of the Judith River. This crocodile, for which the uncouth name *Leidyosuchus canadensis* is suggested, is a broad-nosed type recalling the European Oligocene genus *Diplocynodon*, but with the splenial included in the short mandibular symphysis, as in

some of the *Goniopholididæ*. Possibly it may be a direct descendant of the latter—an idea supported by the fact that several members of the Judith River fauna present a Jurassic *facies*.

The paper of widest general interest on this group of reptiles is perhaps one by Dr. O. P. Hay on the habits and pose of the members of the sauropodous group, as specially represented by *Diplodocus*, published in the October number of the *American Naturalist*. The skeletons of these reptiles, as exemplified by the cast of that of *Diplodocus* in the British Museum, are generally set up as if the creature carried its body raised high above the ground after the fashion of a rhinoceros; while the food of the species in question has been supposed to be the leaves of large succulent water-plants.

Dr. Hay, on the other hand, regards *Diplodocus* as an amphibious reptile, which could swim with facility, and could also probably creep, albeit somewhat laboriously, on land. When feeding it would appear to have swum or crept lazily about, gathering in plants floating on the surface or growing loosely on the bottom. Deep-water vegetation could be reached by means of the swan-like neck, as could also foliage twenty feet above the surface. Perhaps floating algæ, such as *Chara*, which could easily be raked in by the peg-like front teeth, may, however, have constituted the staple food of the *Diplodocus*. That these reptiles ever reared themselves up on their hind-limbs is as unlikely as that crocodiles could perform the same feat.

If these creatures curved their bodies in rhinoceros-fashion, Mr. Hay is of opinion that, with a weight of something like twenty tons, they would inevitably have been mired when walking on marshy ground; and he therefore presumes that the limbs were directed outward, after the manner of crocodiles, with the belly resting on the ground. And he further suggests that the skeletons should be mounted in museums in this pose, when the body, placed in a crocodile-like attitude, would be little, if any, less imposing than when erect; while the long neck, as flexible as that of an ostrich, might be placed in any one of several graceful positions.

It may be mentioned, in connection with these suggestions, that the iguanodon managed to get about, without becoming bagged, on yielding soil, despite the fact that its enormous weight was concentrated on a single pair of supports.

To the *Proceedings of the U.S. National Museum* (vol. xxxv. p. 351) Dr. Hay has contributed another paper on dinosaurs, dealing in this instance with the carnivorous type. The paper is largely concerned with matters of nomenclature; but attention is specially directed to the skull of *Ceratosaurus nasicornis* as the best-preserved example of the cranial osteology of this group of reptiles at present known. Among the most remarkable cranial features is the apparent bordering of the internal nasal passage almost entirely by the palatines, and only slightly by the vomer. In any case, the vomer takes a much less important share in the border of the posterior nostrils than is the case in the tuatera and lizards. In a short article published in *Science* for October 10, the same author criticises Dr. Holland's interpretation of the skull of *Diplodocus*, and proposes other determinations.

The British Jurassic crocodilian fauna has been augmented by the identification from the Oxford Clay near Peterborough of remains referable to *Metriorhynchus brachyrostris*, a member of the unarmoured marine metriorhynchine group previously known only from the equivalent formation of the Continent. These remains, which include a nearly complete skull, have been described by Mr. E. T. Leeds in the *Quarterly Journal* of the Geological Society of London. The skull shows that the vomer extends backwards nearly to the sphenoid—a feature probably common to the group generally.

The only work relating to fossil snakes appears to be the identification by Mr. M. Leriche, in a paper published at Lyons, of remains of the genus *Palaeophis* in the Nummulites of Aude.

For several years past Dr. J. C. Merriam has been working assiduously at the Triassic representatives of the Ichthyosauria, and the full results of his labours have been published in a richly illustrated quarto, forming No. 1 of the first volume of the *Memoirs* of the University of California. After an exhaustive review of their history and their geographical and geological distribution, the author points out the features by which these reptiles differ from their Jurassic and Cretaceous successors. These he finds to be nearly all connected with adaptation to an aquatic existence, as indeed are most of the characteristics by which the entire order is usually defined. As might be expected, these adaptive modifications are less

strongly expressed in the earlier than in the later forms; that is to say, the Triassic species depart less widely in structure from land-reptiles than is the case with those of the Oolites and Chalk. All the evidence indicates that the trend of specialisation in the group has been to develop such features as are of importance for the needs of a purely aquatic life, and to suppress such structure as is of little or no value in this respect. Corresponding in the rôle of Mesozoic nature to the cetaceans of the present day, the ichthyosaurs appear to have been evolved from a more or less ordinary type of reptile with limbs constructed mainly, if not entirely, for terrestrial progression.

Derived from this unknown crawling ancestor, the genus *Cymbospondylus* of the Middle Trias marks a stage in which the reptiles of this group had already abandoned the shore as their regular habitation, but still retained sufficient structural characters to give a clue to the origin of ichthyosaurs in general. Contrasted with the Triassic type we have, at the other end of the chain, such forms as *Baptanodon* and *Ophthalmosaurus*, in which adaptation to a deep-water existence has been carried to such a degree as to have suggested to earlier investigators that these creatures were the least specialised of all reptiles. As regards the date of the evolution of the group, this can scarcely have been later than the early part of the Triassic, and may possibly have been still earlier. The ichthyosaurs are therefore one of the oldest groups of reptiles, and may be derived from one of the very primitive members of the class. Except in the aforesaid characters indicating adaptation to an aquatic environment, the ichthyosaurian skeleton is indeed of an extremely primitive type; but to what group it presents the nearest affinity is, in Dr. Merriam's opinion, still uncertain.

Before leaving the group attention may be momentarily directed to a letter from Dr. W. J. Holland in *Science* for January 31st, reaffirming the fact that the supposed toothless character of *Baptanodon* is based on an error.

A memoir entitled to rank on the same high platform as Dr. Merriam's is one by Dr. O. P. Hay on the fossil Chelonians of North America, issued as No. 75 of the quarto publications of the Carnegie Institution of Washington, and comprising no less than 568 pages of letterpress, accompanied by 113 plates. The memoir is entitled the *Fossil Turtles of North America*, the term

"turtle" being apparently applied in the States to any member of the chelonian order, and not restricted, as in this country, mainly or exclusively to the marine species. The study of American fossil tortoises (to use the term more generally in use) dates only from 1851, when the late Prof. Leidy named and described four species. Dr. Hay's memoir contains an account of no less than 266 species, of which 76 are described for the first time. The author has some interesting remarks with regard to the origin of the chelonians: that they are not derived from the Cotylosauria or Pelycosauria seems evident, owing to the apparent absence in that group of a ventral armour, which was certainly wanting in *Diadectes*. The genus *Placochelys* of the European Trias has been regarded as ancestral to the Chelonia; but, although the body is short and broad and curved above with an armour of small, closely approximated plates, there is no ventral shield. And it accordingly seems that we must look to the labyrinthodonts (stegocephalians) as the ancestral stock.

"From *Placochelys*," writes Dr. Hay, "we learn that in Triassic times there were broad and sharp-bodied reptiles of tortoise-like form and covered with an armour of small bony plates. From *Diadectes* and *Otocolus* we discover that in Permian times there were more elongated reptiles, in the skulls of which there were many chelonian characteristics, and whose bodies were protected by an armour of elongated plates overlying the ribs. The stegocephalian genus *Archegosaurus*, of the Permian, possessed a ventral armour of elongated plates. We may confidently expect that in the Permian there will yet be found an animal possessing such a combination of these characters, together with other features, that we can recognise in it the ancestor of the order of turtles."

Neither does the author attach much importance to the theory that the chelonians are closely allied to the plesiosaurs. The most that, in his opinion, can be affirmed is that possibly the ancestral stocks of the two orders were somewhat nearer to one another than those which gave rise to the other orders of reptiles.

In connection with the supposed affinity of the Chelonia to the Sauropterygia (plesiosaurians) reference may be made to a paper by Mr. R. L. Moodie on reptilian epiphyses, published in the *American Journal of Anatomy*, vol. vii. p. 443. It has been stated that the limb-bones of both chelonians

and sauropterygians are furnished with terminal epiphyses; but, according to the author, such structures are present in neither of the two groups. The long cone-like structures in the humerus and femur of the Sauropterygia, which have been described as epiphyses, are regarded by Mr. Moodie as really parts of the diaphyses of the bone, corresponding with homologous elements found in all other Sauropsida.

Dr. Hay (*Proc. U. S. Nat. Mus.*, vol. xxxv., p. 161) has supplemented his big memoir by a paper on five species of North American fossil tortoises, of which four are new.

Turning to the more generalised orders, a remarkable skull from the Permian of Texas has been referred by Dr. W. D. Matthew (*Bull. Amer. Mus. Nat. Hist.*, vol. xxiv. p. 183) to the Pelycosauria, with the designation *Tetraceratops insignis*. Its most conspicuous feature is the presence of two pieces of prominent bones, or "horns," rising respectively from the premaxillæ and the prefrontals. The skull apparently indicates a reptile allied to *Clepsydropus* and *Dimetrodon*, and therefore probably provided with a tall dorsal fin, supported by the elongation of the spines of the vertebræ of the trunk. In habits the creature, judging from its dentition, was certainly predaceous.

The eighth part of vol. iv. of the *Annals of the South African Museum* contains no less than five papers by Dr. R. Broom on the Permo-Triassic tetrapodous vertebrates of Cape Colony and the adjacent districts. In the first the genus *Propappus*, originally named from a single limb-bone, is considered by the author to be distinct from *Pariasaurus*, having, among other peculiarities, a dermal armour on the spinal region. New generic types of the carnivorous groups of anomodont, or mammal-like, reptiles are also described; and it is pointed out that the difference in the structure of the palate between the Permian and the Triassic representatives of these reptiles amply justifies their separation into distinct groups. While the latter, as represented by *Galesaurus*, have a typically mammalian secondary palate, that region in the former is a modification of the type obtaining in the rhynchocephalian reptiles. For these two groups Dr. Broom employs the names Cynodontia (=Theriodontia) and Therocephalia. In the last paper of the series the author assigns certain Cape labyrinthodonts (one of which had been previously referred to the American *Eryops*)

to the new genus *Rhinosuchus*, of which, however, the type is a German species.

One of the foregoing determinations is unlikely to remain unchallenged, for in a paper published in the Zoological Society's *Proceedings* (1908, p. 605) Professor H. G. Seeley considers that *Pariasaurus* itself carried a dermal armour, taking the form of one median and two shorter lateral rows of ovate bony scutes along the back.

In a second paper published in the same journal (p. 611) Professor Seeley gives additional particulars concerning the skull and dentition of the South African anomodont genus *Diademodon*, in which the cheek-teeth are regarded as presenting remarkably mammal-like features.

At the close of a communication on the origin of the mammal-like reptiles contributed to the Zoological Society's *Proceedings* for 1907 (p. 1047), Dr. Brown remarks that

The mammal-like reptiles form a well-defined group whose earlier members show so much affinity with the primitive diaptosaurians and with the higher cotylosaurians as to render it highly probable that from some cotylosaurian ancestor all the later reptiles are descended. On the other hand, the higher mammal-like reptiles approach so closely to the mammals that it is not always possible to distinguish between them. *Tritylodon* is held by many to be a reptile; by others it is believed to be a mammal. *Dromatherium*, *Microconodon*, and *Karoomys* are generally believed to be mammals, but it is just possible they may be reptiles; while *Sesamodon* and *Melinodon*, which are believed to be cynodont reptiles, may possibly prove to be mammals. The difference between a cynodont reptile and a monotreme is less than the difference between a monotreme and a marsupial, and again is not much greater than that between a marsupial and an insectivore.

Very interesting is the information that has been recently recorded with regard to the oldest known reptiles. Towards the close of 1907 Dr. Thevenin, in the *Annales des Paléontologie*, 1907, pt. 3, p. 19, described from the uppermost Carboniferous oil-shales of France certain remains which he named *Sauravus costei*, and regarded as the oldest discovered reptile. He considered it as being related to *Palæohatteria* and the other Rhynchocephalia. According, however, to Dr. S. W. Williston (*Journ. Geol.* vol. xvi. p. 395), it is much more probably akin to *Isodectes copei*, an extremely primitive tetrapod from the Coal

Measures of Ohio, showing evident signs of relationship with amphibians, but more probably a cotylosaurian reptile, and if so undoubtedly the most ancient member of the class at present known. As regards the former point, Dr. Williston remarks that if the parasphenoid shows marked signs of reduction in *Sauravus* and *Isodectes*, we must regard those genera as reptiles, even though they retain paired occipital condyles; if, on the other hand, the parasphenoid be fully developed, they must be classed among the microsaurian amphibians. In both genera the number of the phalanges is 2, 3, 4, 5, 4, as in rhyngocephalians and lizards.

On page 139 of the volume already cited Dr. Williston takes the skeleton of another very primitive reptile, *Labidosaurus incisivus*, from the Permian of Texas, as the text of an elaborate dissertation on the affinities and structure of the Cotylosauria, of which group he gives a full definition. His figure shows the skull of *Labidosaurus* to be roofed over in the labyrinthodont style; and the author regards the genus as related to both the Scots *Telerpeton* and the South African *Procolophon*—genera which have been widely sundered by other writers. The skeleton of a second species of the same genus, *Labidosaurus humatus*, is described by Mr. Broili in the *Zeits. deutsch. geol. Ges.*, vol. lx. p. 63.

In regard to Amphibia, the only paper of any importance appears to be one by Mr. Moodie (*Journ. Morphol.*, vol. ix. p. 111), on the lateral line system of the stegocephalians, in which it generally occurs in the form of a "lyra" on the skull. In the Branchiosauria, however, it forms a line on each side of the tail, as in the modern *Necturus*; while in the Aistopoda it is absent. The maximum development occurs in the stereospondyli, in which the canals are the smoothest; this being a sign of age and also of specialisation. Apart from certain differences, some of these head-canals can be homologised with those of fishes like *Amia*. Definite names are given to the constituents of the "lyra." Finally, it is stated that the bone originally named squamosal in stegocephalians is really that element and not the supratemporal.

In the class of fishes reference may first of all be made to papers of a faunistic type, one of which—on the Cretaceous fish-fauna of Brazil—was contributed by Dr. Smith Woodward to the *Quarterly Journal of the Geological Society* (vol. lxiv. p. 358),

containing descriptions of several new species, but nothing of special interest. The Palæontographical Society has issued (up to the close of 1907) two more contributions to the history of the British fish-fauna, namely the third part of Dr. Smith Woodward's *Fossil Fishes of the English Chalk*, and the first part of *A Synopsis of the Carboniferous Ganoids*. In the former the author treats of the great carnivorous "barracudas," like *Porthenus* and *Elops*, among which he describes the new genus and species *Dinelops ornatus* from Kent and Surrey.

As regards other additions to the British fossil fish-fauna, Mr. J. F. Jackson (*Geol. Mag.*, decade 5, vol. v. p. 309) has recorded the occurrence of several well-known Carboniferous species from a limestone-quarry near Sparrowpit, in North Derbyshire. Remains of a representative of the American bowfins (*Amia*) have been described by Mr. M. Leriche (*Bull. Soc. Belge Géol.*, vol. xxii. p. 121) from the Hempstead beds of the Isle of Wight. Last year Mr. Leriche identified remains of a bowfin (*Amia eocena*) from the Lower Eocene of Suffolk, which had been regarded as lacertian; while the genus is also known from the Upper Eocene of Hampshire, as well as from the Osborne and Bembridge group of the Isle of Wight.

Reverting to papers of a faunistic type, Mr. Leriche, in a pamphlet issued by the University of Lyons, records a number of well-known species from the Nummulitic formation of Aude (Corbières Septentrionales). Fish-remains from the Upper Cretaceous of Hasseinabad (Poucht-i-Kouk), Persia, are described by Mr. F. Priem in a volume entitled *Annales publiées sous la direction de J. de Morgan*, at Paris. The specimens forming the subject of the memoir were collected by Mr. de Morgan during a recent expedition to this part of Persia.

In *Mem. Geol. Surv. New S. Wales, Palæont.* No. 10, Dr. Smith Woodward has described fish-remains from two horizons of the Hawkesbury beds of St. Peter's. Most of the species belong to European genera, and indicate a Permo-carboniferous age for the formation. The new genus *Elpisopholis* proves intimate relationship between the *Palæoniscidæ* and the *Belonrhynchidæ*, indications of which were previously afforded by a species of *Belonrhynchus* from the Hawkesbury beds of Gosford.

In systematic work reference may be made to a paper by Mr. C. Fraipont, published in the *Ann. Soc. Géol. Belge*, vol. xxxv.,

containing the description of a new species of armoured fish-like creature, *Pteraspis dewalquii*, from the Gedinnien stage of Belgium, and likewise a notice of a remarkable type of ventral shield from the Taunusien schists assigned to the British *Pt. crouchi*.

An important faunistic paper is one by Dr. Lucien Maget, on the Miocene mammals from the sands of the Orléannais, and the Faluns of Touraine, published in the twenty-fourth volume of the medical section of the *Annales de l'Université de Lyon*. Full descriptions are given of remains of all the previously known species, while nine species are described as new.

THE EVOLUTION OF ANIMAL FUNCTION

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PART I

No serious student of biological thought can have failed to observe how small a part the hypothesis of evolution has played in the development of animal physiology. In every other department of biology the publication of the *Origin of Species* marks a profound change in the direction of research and an altered interpretation of facts already known. To the study of animal structure, for example, Darwin's work brought a vigour and activity such as had never before been experienced. Morphologists found a new meaning in their work. They ceased to toil at a classification whose basis was mere caprice, tracing instead the sequence of a course of development which bound together the whole animal kingdom by the tie of common blood. But with the study of animal function it was otherwise. There is no break in the history of physiology to mark the pre-Darwinian from the post-Darwinian period. Questions of function have never been called in to help in tracing the course of evolution, and the idea of evolution has given no aid in the interpretation of the known facts of function. If the hypothesis of evolution were to-morrow to be proved untenable, physiologists would scarcely be concerned.

We naturally inquire for the cause by which two sciences so closely related have been led into such diverse paths. And it is not far to seek. Many years before Darwin wrote, the study of morphology had taken upon itself the duty of classifying animals, pronouncing definitely against the claim of physiology to a share in the enterprise. And it followed in the regular course of things that the criteria of classification afforded the material by which the developmental relationships of animals were afterwards determined. In this historical fact there lies, as I believe, the key to that indifference which physiologists still display towards the evolution of those functions which form the

matter of their science. Of course it will be urged that the quarrel over the use of functional characters in classification took place a century ago, and is long ago forgotten; that no biologist now takes seriously or even remembers the arguments by which Geoffroy St. Hilaire established the monopoly of morphology. All this may be conceded. Nevertheless I would maintain that in the divorce of evolution and physiology there is something more than the mere preoccupation of physiology with the pressing claims of medicine.

It is not implied that physiologists are at this present day directly prevented from developing their science on comparative lines by any views that may now be held as to the value of functional characters as a test of animal relationship. The point is rather that the original exclusion of functional characters from the criteria of classification robbed physiology of that compelling stimulus which has made morphology a comparative science. When the failure of physiology to take any part in animal classification or in the determination of genetic relationship is attributed to the preoccupation of physiologists with more immediate problems, it is implied that the old quarrel between structural and functional characters played no part in the matter. But in such a view of the case there is in all probability a confusion of cause and effect. It may well be maintained that the preoccupation of physiology with the study of man, and its failure to develop on comparative lines, were simply the outcome of its exclusion from the problem of animal classification.

It will be of interest to attempt at this stage to recall the history of those events which resulted in the exclusion of functional characters from the determining of animal relationship. For if no other purpose be served, we may at the least hope to see how widely the modern conceptions of function differ from those which were held in the time of Cuvier. And so we may be led to a clearer formulation of the problems which now await solution from the comparative study of function.

It was about the middle of the eighteenth century that the organisation of animals began to be treated on a definitely and consciously comparative basis. Such men as Daubenton in Paris and John Hunter in London set before them, as the object of their study, the comparison of animals one with another. And naturally enough they took as the matter of

comparison any distinctive characters which the animals might present, regardless whether they dealt with differences of structure or of function. This comparative work afforded the material with which subsequent workers began to build up a classification of the animal kingdom. Accordingly we find the early classifiers using indifferently likeness of function and of structure. So, for example, Vicq d'Azyr, who was Professor of Anatomy in Paris from 1774 to 1794, takes as a ground for classification not only structural similarities between animals, but also the presence in them of organs, such as those of respiration, whose likeness is purely one of function.

This method survived well into the beginning of the nineteenth century. Dugés, a prominent member of the French school who wrote about 1830, speaks of the species as "*un type idéal de forme, d'organisation, de mœurs.*" In much the same way Cuvier's classification of the animal kingdom was partly morphological and partly physiological. He took as characteristics certain organs which are distinguished from others, not by their structure or anatomical relations, but by their functions only. He even went so far as to lay particular stress upon a structure as a ground for classification, if its function was of more than ordinary importance. This attitude is well expressed in his own words: "*toutes les parties, toutes les formes, toutes les qualités d'un organe ne sont pas également propres à fournir des caractères pour les classes supérieures; ce sont seulement celles de ces formes et de ces qualités qui modifient d'une manière importante la fonction à laquelle cet organe est affecté.*"

Now, in so far as Cuvier or any other zoologist used the characters of animals simply and directly as a test of their likeness, and consequently of the propriety of their being classed together, it could be of no importance whether the characters used were matters of function or of structure—one point of likeness will serve as well as another to mark the similarity. But to this direct method there soon succeeded one which depended upon the philosophical doctrine of the common plan of organisation of animals. This idea was strongly developed at the beginning of the nineteenth century by Oken and Goethe, with other members of the imaginative German School of "*Naturphilosophie*," and gained a great hold over the biologists of the time. As a corollary of the

doctrine it was held that in different animals there might be traced parts which represented, though often with considerable modification, the same constituent of the ideal plan of organisation; and these corresponding parts came to be called homologous.¹ Working under the influence of this doctrine, zoologists began to base their classification of animals on the presence in them of recognisable homologous parts. It was over the criteria of homology that the split between structural and functional characters took place.

Cuvier recognised this doctrine of a common plan of organisation, and made use of it in working out his system of classification. His method was to trace throughout the animal kingdom organs which correspond in function, and to find how far they were like or unlike in structure. That the method was not wholly satisfactory his own words show: "On voit . . . que ce qui est commun à chaque genre d'organes, considéré dans tous les animaux, se réduit à très peu de chose, et qu'ils ne se ressemblent souvent que par l'effet qu'ils produisent." He illustrates this fact particularly from the organs of respiration, which, as he points out, differ so widely in different classes of animals that they often present no common point of structure whatever. That the method was actually wrong was first maintained by Geoffroy St. Hilaire. He showed that parts of an animal which did not correspond at all in function had often an underlying correspondence of structure. He pointed out, for example, that the organs used by vertebrates for the different functions of running, climbing and swimming—namely, the fore-legs of mammals and the pectoral fins of fishes—differ widely in function, but present on analysis a correspondence of structure. Therefore, he argued, likeness of structure is the true criterion of homology, while function is an adventitious property marking merely the use to which the corresponding or homologous parts of each animal are applied. It would follow that as long as classification proceeded under the influence of the doctrine of a common plan of organisation, by making the presence of homologous

¹ I use the word "homologous" here to avoid confusion, because it is now used in this sense. As a matter of fact, Geoffroy St. Hilaire, who first gave a clear definition of the relation which we now term homology, used the word "analogous." But analogous now means in zoological terminology like in function, which is the very meaning which Geoffroy wished to exclude from his definition.

parts the basis of comparison, so long the sole criterion of classification must be found in likeness of structure. Questions of function must be rigidly excluded. This conclusion is expressed clearly by Geoffroy in the words: "Il est évident que la seule généralité à appliquer dans l'espèce est donnée par la position, les relations et les dépendances des parties."

Up to this point, whether from sufficient or insufficient premises, the position had been logically developed. But not unnaturally zoologists came in course of time to forget the exact steps of the argument, and to extend its conclusion beyond the proper limits. What had been established was that, so long as classification proceeded by the tracing of homologies, structural characters alone could serve as the basis of classification. Beyond this the assumption was tacitly made that the method of homology was the only possible one in classification. Accordingly, classification became completely identified with the comparison of structure, while any possible use of functional characters was tabooed.

It is difficult to see, if one disregards the results which the two methods achieved, why the system of Geoffroy should have been accepted rather than that of Cuvier. Geoffroy found likeness of structure transcending difference of function, while Cuvier treated likeness of function as though it were more fundamental than difference of structure. It is not inconceivable that biology might have developed along the lines of a common plan of function. Complexity of functional arrangement might have been made the basis of classification, the structure which served each function being regarded as of subsidiary importance. But the doctrine of a common plan of organisation had a very strong hold on zoologists, and it was essentially a doctrine of structural likeness: as developed by the school of "Naturphilosophie" it amounted almost to an ideal geometry of animals. We have to reckon also with the overwhelming evidence which soon accumulated, showing the fertility of the method of structural homology. Under its influence classification grew rapidly into a consistent science. Finally, the method seemed to receive complete confirmation when the embryonic development of animals came to be studied. For it was then found that many structures which had previously been recognised as homologous were actually developed from

corresponding elementary parts of the early embryo.¹ So the claim of structural characters to form the sole criterion of homology, and hence of classification, appeared to be established.

For the rest the story is a simple one. When the idea of evolution was formally enunciated by Darwin, and so became a working hypothesis, the old methods of classification were accepted without inquiry as the methods by which the course of evolution must be traced. The relationships of animals were determined solely on the basis of structural homology. No more striking evidence could be found for the completeness with which questions of function had been excluded from all inquiry into the relationship of animals.

So it has come about, mainly through the work of Geoffroy St. Hilaire, that the study of animal function is almost wholly separated from the study of evolution. The separation is greatly to be deplored. For in the first place it is not improbable that the relationships of animals might have been better determined if morphology had co-operated with physiology in the work. And beside this, a point which has been of far greater importance to biology, the study of the evolution of function, might have developed, given only the requisite stimulus which the tracing of genealogy would have supplied, into a body of knowledge of far greater moment for the understanding of evolution than any purely structural study can ever be. If physiologists had felt that the comparative study of function could form a science really essential to the understanding of evolution, it is hard to believe that they would not have hastened to remove the reproach so commonly made against them, that their animal kingdom comprises only the frog, the rabbit, the cat and the dog. It is easy to urge by way of excuse and explanation that a study of the functions of invertebrate animals demands a technique which is difficult and little understood. But this is merely to state the same fact from the opposite side. The technique of experiment on invertebrate animals is not in itself essentially difficult; it has merely failed to develop because the doctrine

¹ The whole question of the validity of "homologies" determined on the ground of embryonic development has been raised in recent years by such workers as Wilson, Braem, and Gaskell. The question is one of particular interest because it touches the fundamental distinction of structural and functional characters. But space forbids any discussion in this place.

of structural homology deprived comparative physiology of its chief aim and incitement.

Physiology has advanced since Geoffroy wrote to a depth of functional analysis of which he can have formed no conception. It is of interest, therefore, to examine Geoffroy's conclusions from the modern standpoint. Is it true in the light of our present knowledge that questions of function can have no voice in deciding the line which animal genealogy has followed? And, even if this prove to be so, may there not be an independent science of the development of function, whose value will lie in its direct contribution to the understanding of evolution? These are questions which surely demand our attention.

The first point to be settled, in order to place on a sound basis the discussion of the use of functional considerations in the tracing of evolution, is the exact definition of what is meant by function as used in this connection. A glance at Geoffroy's classical example of likeness of structure transcending difference of function shows us at once the shallowness of his analysis. He pointed out that the fore-legs of mammals and the pectoral fins of fishes correspond in structure, though they perform such diverse functions as running, climbing and swimming. But to us this difference seems scarcely a difference of function at all. We are accustomed now to think of function in connection with the individual units by which it is performed—namely, the muscle cells, the nerve cells, the gland cells, and so forth—which are devoted each to the performance of their peculiar function. And as far as the individual functioning cells are concerned, a leg used for running need hardly differ from a fin used for swimming. It is possible to picture the change from one to the other taking place without any change whatever in the functional behaviour of the cells. In each the nerve fibres might conduct the nervous impulse similarly, and the muscle fibres might be excited and contract in an identical manner. And, indeed, the difference resolves itself, in so far as it is not simply one of structure, into a different control of the organ by the central nervous system. It may amount to no more than a different sequence of the identical events of conduction and contraction which the central nervous system determines. The first point, then, to be emphasised is that no confusion must be made between

the general use or application of an organ and the functional events which go on in the individual cells of which it is composed.

But even when a function has been referred to the individual cell by which it is performed, analysis has not yet gone deep enough. There is still a possibility of confusion between two quite independent factors which determine the functional behaviour of any cell. The actual functional behaviour of a cell is not settled entirely by the properties of the cell itself. It is the product of the capability of the cell to respond to a given external cause by which it is disturbed, and of the particular disturbing cause which is acting at the time. This distinction between functional behaviour and capability may sound trite and obvious; but it is too easily slurred over in the treatment of the evolution of function. We must therefore digress at this point in order to define the matter more clearly.

The cells which constitute an organism react one with another in many different ways. Their mutual reaction constitutes the whole system of co-ordination or "integration" under whose working a conglomeration of unlike cells takes on the character of a regulated organism. A nerve cell may act upon another nerve cell or upon a muscle cell or gland cell by the direct means of the nervous impulse. One cell of the digestive mechanism may pour into the blood-stream a chemical secretion which is carried to and promotes the activity of other digestive cells. Fatigued muscle cells may deliver to the blood a product which passes to the brain, there to throw into activity certain nerve cells, and from these nerve cells impulses may pass down to control the muscle cells of respiration. All these are examples of the co-ordination and direction of the functional behaviour of one cell by the activity of another. And it is clear that this co-ordination involves the entire activity and structure of the organism. It depends, of course, upon the sequence and time relations of the individual activities of the cells concerned. But it involves also the anatomical relations of organs, their blood supply, and the paths of their nervous connexion within and without the central nervous system. Evidently, then, a difference of behaviour between two cells, in so far as it may spring partly from the reaction with other cells, cannot be thought of as a pure difference of function. It may be essentially a difference of structure, a rearrangement of nervous connection, for example.

So that if we were to take normal behaviour as the basis of comparison between different cells we should soon be floundering in a hopeless confusion of structural and functional characters. And even if the particular case which we might consider were one into which structural considerations did not enter, still the behaviour of any cell examined would probably be only in part the outcome of the cell's own properties, and for the rest would depend on the properties of other cells contained in the organism. It would in fact be impossible to proceed on this basis with any sort of certainty to compare two types of cell in the matter of their individual functions.

Therefore we need to form a conception of the functional capability of a cell as distinguished from its normal behaviour. Two cells must be considered to differ in functional capability only if they react differently from like stimuli when placed under identical conditions of environment. A concrete example may make this point clearer. The cells which compose the heart-muscle of a frog contract during life with a regular rhythm, relaxation following contraction and contraction relaxation at fixed intervals of time. The cardiac muscle cell of the lobster behaves in a manner almost identical. The normal behaviour of these two cells is therefore similar. But if we place the two under identical conditions and subject them to like stimuli it becomes clear that they differ considerably in functional capability. When subjected to a series of rapidly repeated electric stimuli the cardiac muscle cell of the frog exhibits a refractory phase, immediately succeeding each contraction, during which the stimuli are unable to evoke any further contraction whatever. The cardiac muscle cell of the lobster, however, remains in complete contraction as long as the series of stimuli continues; it presents no refractory phase. This is an example of cells alike in behaviour but different in functional capability. Similarly one can find examples of the converse—difference of behaviour with likeness of capability. Behaviour is in fact so little coincident with functional capability, that two cells may possess a difference of capability which is completely latent, never showing itself in the normal life of the cell as a difference of behaviour. We must insist therefore that the basis of functional comparison between cell and cell shall be the pure functional capability of each, quite abstracted from the similarity or dissimilarity of their normal behaviour under the possible influence of other

cells. In functional capability as here defined we have a true physiological property which belongs wholly to the cell in which it is found.

With this distinction made clear we may return to the main issue and inquire, first, whether the functional capabilities of the cells which constitute an organism may legitimately serve as a guide to the course of evolution which the organism has followed. The real question which this involves is whether functional capability is a property which remains constant over considerable periods of evolution, or whether it is subject to frequent adaptive changes following every alteration of environment and habit. Towards the solution of this problem we might proceed in two different ways. We might inquire directly to what extent likeness of functional capability runs parallel throughout the animal kingdom with the course of phylogenetic development already determined by the usual structural criteria; or we might consider how far functional capability is determined directly by inheritance, and how far it is the result of subsequent influences to which the cells are subjected in the course of an organism's development. In the present state of our knowledge it is not possible to apply the first of these two methods. We have but little evidence as to the degree of difference which functional capability exhibits throughout the animal kingdom. Such experiments as have been made on the functions of individual cells have not been directed in the rigid manner which the investigation of pure functional capability would demand. We are bound, therefore, for the immediate purpose of the argument to put together such evidence as the second method can afford.

The problem presents itself in the following form. How far is the functional capability of a cell directly determined by inheritance, and how far is it dependent upon the environment and activity of the organism to which the cell belongs? For the little that we know of the directness of inheritance we have to look to the school of Experimental Morphology, or *Entwickelungsmechanik*. This school has attempted, by the isolation of single cells in the very early stages of embryonic development, to determine how far the structural characters built up in the course of development are the outcome of the individual properties of the single cells from which they arise, and how far they result from the interaction of other cells belonging to the same organism.

The conclusion drawn from such experiment is that there is an early stage of embryonic development during which each cell is pursuing the proper course predetermined by its own inherited qualities; and that to this there succeeds a stage in which the interference of the general functional activity of the organism is necessary to the completion of the process. In this distinction we get the first glimpse of directness and indirectness of inheritance. Characters determined by the first stage are more directly dependent upon inheritance than those determined during the second. What evidence is there, then, that the functional capabilities of cells are determined rather in the direct than in the indirect stage of development? On this point the evidence is scanty. One can discover here and there an isolated observation, such as that made by Wintrebert, for example, that the muscle cells of the developing frog show a capability of contraction in answer to mechanical stimuli at a time when they are not yet in functional connection with the central nervous system. Such observations as this suggest that functional capability is in some cases at any rate determined at an early stage of development. There is no indication that it is a property the traces of whose phylogenetic development will be easily wiped out. In this connexion the importance of the distinction between functional capability and functional behaviour becomes even more apparent. For the latter, being dependent upon the activity of cells other than those by which it is immediately exhibited, may well be open to frequent adaptive modification determined by the environment of the organism; while the former appears, from the evidence quoted above, to be capable of establishment at a period of development when such interference is not yet possible.

We might carry this discussion on to the further point, how far the use of functional capability might legitimately be extended to the comparison of animals under the method of homology, and how far it should be limited to the direct method of comparison. But it is obvious that any conclusions which might be reached in the present state of our knowledge would rest upon such slender evidence that their value could not be great. The one patent fact is that we know scarcely anything at all about the functional capabilities of cells, whether as regards development in the embryo or distribution through

the animal kingdom. And from this fact two consequences would seem to follow: first, that there can be no sufficient grounds whatever upon which any biologist of the past or the present time could deny the legitimacy of the use of functional characters as a test of animal relationship; and, second, that it is high time that the investigation of functional capability should be begun in a conscious and systematic manner.

To this position we have been led from a study of the historical connection between physiology and such attempts as have been made to unravel the relationship of animals. It is but natural, therefore, that the comparative study of functional capability has been treated throughout as though its one aim and justification must be sought in its contribution to animal genealogy. But this, I think, is far from the truth. Such a study, if ever it should advance to any sort of coherence, must draw its chief interest from the direct insight which it would afford into the actual course which has been followed by the evolution of function. At present a deep obscurity surrounds this problem. Physiologists accept the fact that one cell spends its energy in contracting, another in secreting useful ferments, a third in expelling waste products from the organism. But there is hardly a suggestion as to the past history of each type of cell. One thinks loosely of the division of labour and of specialisation, and forgets the ignorance which these words mask. And yet it is difficult to escape the conviction that here, certainly no less than in the study of structure, lies the true history of animal evolution.

Broadly speaking, one may divide the study of functional capability into two great parts—that which compares the cells of different members of the animal kingdom, and that which traces their gradual differentiation in the developing embryo. These are the phylogeny and the ontogeny of function. And it is hard to decide upon which part investigation may most profitably begin. In a subsequent article I hope to discuss this matter, and to attempt besides to indicate the general lines upon which the study of functional capability may best proceed.

A DREAM OF FAIR HYDRONE

(A CHEMICAL IDYL)

BY HENRY E. ARMSTRONG

PROLOGUE

FIRE-DAMP or dampf—the fiery vapour of coal-pits—the explosion of which has often given rise to the most terrible catastrophes, also known as marsh gas on account of its production when vegetation decays in stagnant water, is the simplest *hydride* of carbon, being represented by the formula CH_4 .

This gas is the absolute foundation-stone of organic chemistry. Systematists call it *methane*, on account of its relation to the alcohol present in wood spirit, *methylic alcohol*, $\text{CH}_3(\text{OH})$, which is formed from it by the process of *hydroxylation*, by the displacement of one of its atoms of hydrogen by the compound radicle¹ *hydroxyl*, OH —that is to say, water, HOH , less an atom of hydrogen, consequently worth just as much as the hydrogen atom as a combining unit.

Methane, in comparison with the hydrides of non-metallic elements other than carbon, is strangely lacking in positive qualities. It is gaseous at temperatures far below the ordinary zero, its boiling-point being -138° , not so very far from that of liquefied oxygen (-182°) and nitrogen (-195°) gases, which is clear proof that its molecules have but very little tendency to cling together.

Methane is the initial term of the paraffin series of hydrocarbons the composition of which is represented by the general expression $\text{C}_n\text{H}_{2n+2}$. Paraffin, the white waxy solid, known best in the form of paraffin candles, is so named on account of its indifference (*parum affinis*) to almost all chemical agents, even ozone; the name is equally descriptive of all terms of the series

¹ The term *radicle* is applied not only to the simple atom, but also to a group of atoms which is capable as a whole of exercising the functions of an atom by taking its place.

from methane upwards. Solid paraffin is a mixture of higher terms of the series, the various constituents of petroleum—petrol, burning oil, the mineral lubricating oils and vaselin—consisting for the most part of intermediate terms.

Methane is remarkable among hydrides as that in which hydrogen is present in the highest atomic proportion in which it is known to occur in any simple hydride, the typical hydrides being as follows :

HCl
Hydrogen
chloride

H₂O
Hydrogen
oxide

H₂N
Hydrogen
nitride

H₄C
Hydrogen
carbide

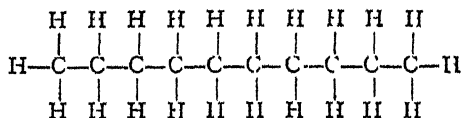
As no compound of two elements is known in which a single atom of hydrogen is associated with more than a single atom of any other element, it is to be supposed that in all these hydrides the several hydrogen atoms are separately attached to the single atom upon which they attend. The valency or atom-fixing power of hydrogen being taken as unity, that of other elements which form hydrides is indicated by the number of hydrogen atoms with which they severally unite.

If it be granted that the hydrogen atom can be united directly with but one other atom—in technical phraseology, that hydrogen is always a monadic or univalent element—it follows from the properties and composition of methane that a single atom of carbon unites with and is satisfied by four atoms of hydrogen; in other words, it is tetradic or quadrivalent. The molecule is practically saturated: that is to say, it has little if any attractive power or tendency to unite with other molecules; moreover, what is true of a single carbon atom and its associated hydrogen atoms is equally true of the more complex systems of which the higher paraffins consist.

And as the hydrogen atom cannot serve as the link between other atoms, it necessarily follows that in the terms above methane the carbon atoms are linked together among themselves and that only their spare affinities are to be regarded as satisfied by hydrogen atoms.

Thus the first homologue of methane—the term next to it in the paraffin series—ethane, C₂H₆, is necessarily regarded as a compound in which two tetrad atoms of carbon are united by one affinity of each atom, leaving six affinities which are satisfied by six hydrogen atoms or C₂H₆ = H₃C · CH₃.

The composition of the higher terms is expressed similarly on paper, *decane*, for example, by the formula :



Such hydrocarbons, it will be obvious, consist of a succession of terms each of the value CH_2 , and of two end terms each of which contains an additional atom of hydrogen : hence the expression of their composition by the formula $\text{C}_n\text{H}_{2n+2}$.

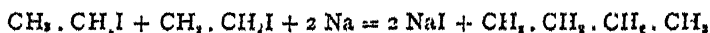
These conclusions as to the constitution of paraffin hydrocarbons, based on the simple assumptions that hydrogen is a monadic and carbon a tetradic element, have been completely verified in practice. For example, if methylic alcohol, $\text{CH}_3(\text{OH})$, be converted by the action of hydrogen iodide into methylic iodide and this latter be subjected to the action of a metal, the iodine is withdrawn and the hydrocarbon residue or radicle CH_3 at once unites with a second such group of atoms formed from another molecule of the iodide :



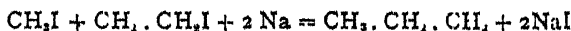
The process is precisely similar to that by which hydrogen gas is formed from hydrogen iodide :



If the iodide prepared from ethylic alcohol, $\text{C}_2\text{H}_5(\text{OH})$, be used, diethyl or tetraene is produced :



Or if a mixture of two iodides be submitted to the action of the metal, the respective hydrocarbon radicles become united ; thus :

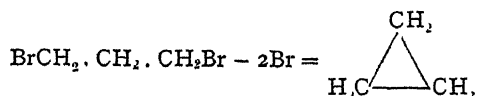


Hydrocarbons of any degree of complexity may be built up in this manner.

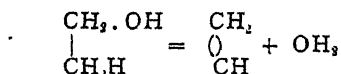
The operation is akin to that of forming a simple chain by forging link upon link ; on this account, such conjunctions of simple hydrocarbon radicles are spoken of as open chain hydrocarbons. Apparently there is no limit to the length of such *open chains*.

But *closed chains or rings* of carbon atoms may be formed by

linking the ends of open simple chains together. For example, if trimethylene bromide, $\text{BrCH}_2 \cdot \text{CH}_2 \cdot \text{CH}_2\text{Br}$ (which is to be regarded as propane, $\text{CH}_3 \cdot \text{CH}_2 \cdot \text{CH}_3$, in which an atom of hydrogen in each of the CH_3 groups is displaced by an atom of bromine) be subjected to the action of sodium, it is not only deprived of the two bromine atoms, but the two ends of the chain become linked together :



Even two carbon atoms may be united in a closed ring, as in ethene or ethylene, which is formed by mixing alcohol with concentrated sulphuric acid and heating the mixture :



But apparently it is not possible to convert every open chain hydrocarbon into the corresponding ring system—the most complex compounds of this kind known at present contain only seven atoms of carbon.

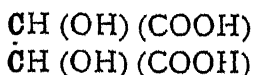
The recognition of such limitations necessitates the recognition of structural peculiarities in the carbon atoms, the effect of which is to limit their activity, so that they can only be arranged in a certain limited number of ways. It is important that these peculiarities should be noticed.

A model or symbol in remarkable accord with the general chemical behaviour of carbon is afforded by the regular tetrahedron. Assuming such a tetrahedron to be inscribed inside a sphere, the centre of influence of the carbon atom may be supposed to be at the centre of the sphere and the four affinities of the atom may be thought of as directed outwards towards the four solid angles of the tetrahedron ; in this arrangement, the angle included between two affinities is $109^\circ 28'$.

Such a model is easily constructed by joining together at their edges four equilateral triangles cut out of cardboard. But for the purpose of studying the manner in which carbon atoms can be united together, it is better to bend two pieces of wire at an angle of $109^\circ 28'$ and then to solder these together at the bend

in such a way that the plane passing through the two arms of the one is at right angles to that passing through the two arms of the other. The four arms should be made of equal length. Such model carbon atoms can be united by juxtaposing an arm of each of two skeleton tetrahedra and then binding these together. Provided they be arranged in spiral order any desired number may be united in such a manner, but the number which can be arranged so that their centres lie in one plane is strictly limited: five naturally form a ring which is practically complete; *any less number gives an incomplete ring*: the significance of this peculiarity will be discussed later on. Models of the higher paraffins made with such skeleton tetrahedra present the appearance of curls rather than of straight chains.

The adoption of this tetrahedral conception of the functional activity of a carbon atom is justified in the most absolute manner possible by the discovery which is so indelibly associated with the name and fame of the great Pasteur—namely, the discovery of two modifications of tartaric acid identical in composition, which are distinguishable only by a minute difference in crystalline form and by their behaviour in polarised light: a solution of the one having the power of deflecting a beam of polarised light just as far to the right as an equally concentrated solution of the other will to the left. In tartaric acid



each of two carbon atoms—those printed in clarendon type—is associated with four different radicles H, (OH), (COOH) and [CH(OH)(COOH)].

If two large cardboard tetrahedra be taken, the one without one of its sides so that it can be superposed upon the other, it is easy to fix four differently coloured caps over the four solid angles of each tetrahedron so that when the two tetrahedra are superposed like colours occupy like positions on each. But take off the capping tetrahedron and place the other in front of a mirror or looking-glass: note the position of the coloured angles in the image and assume that the tetrahedron used as cap is in the position of the image, then arrange the colours at its angles so that they are in the position which they occupy in the image; superpose this capping tetrahedron on

the other: the two no longer fit in the sense that the coloured angles match. In fact, two non-superposable models are obtained; but it is only when all four angles are differently occupied that two non-superposable forms are possible.

Applying this argument to the atom itself, the conclusion is arrived at that a compound such as tartaric acid in which the four affinities of a single carbon atom are satisfied by four different units or radicles may exist in two forms: the fact that these are optically active to equal extents but in different directions being accounted for by the spiral arrangement in space in reverse order of the four radicles in the two cases.

A carbon atom in the condition pictured is termed an *asymmetric* atom, as such a system as that described has no plane of symmetry—in other words, it cannot be cut into halves. Asymmetric carbon plays a dominant part in Nature; it may, indeed, be said that it is the Rosetta stone with the aid of which it is becoming possible to arrive at an interpretation of the fundamental phenomena of life in so far as these can in any way be made clear by the chemist.

ARGUMENT

The hydrides of chlorine, oxygen and nitrogen—hydrogen chloride, hydrogen oxide and hydrogen nitride (ammonia)—are compounds the behaviour of which is in striking contrast with that of the simplest hydride of carbon. Three of these four hydrides are gaseous far below ordinary temperatures:

	HCl	H ₂ O	H ₂ N	H ₂ C
Mol. wt.	36.4	18	17	16
B.P.	-83°	100°	38.5°	-138°

hydrogen oxide alone liquefying without compulsion. The degree of readiness with which they pass into the liquid state obviously, however, bears no relation to their molecular weights, which differ but slightly in three cases, the fourth compound (hydrogen chloride) being intermediate in volatility between the two which are less than half as dense.

The mere fact that hydrogen oxide becomes water so readily must be held to be proof in itself that the molecules of the oxide are gifted in a high degree with attractive forces which lead them to combine *inter se*. No such evidence of mutual

affinity is met with either in the case of hydrogen chloride or in that of ammonia; but although these have no tendency to fall victims to self-loves their affection for water is extraordinary, the former being soluble to the extent of nearly 500 volumes and the latter to the extent of about 1,100 volumes in one volume of cold water. A solution of hydrogen chloride saturated at 0° contains about 82 parts of the chloride to 100 of water, corresponding to the ratio $\text{HCl} : 2.4\text{OH}_2$. A solution of ammonia saturated at 0° contains about 87 parts of the nitride per 100 of water, corresponding to the ratio $\text{NH}_3 : 1.1\text{OH}_2$. Hydrogen oxide also, it may be pointed out, is undoubtedly soluble in water at all temperatures; but it is impossible to measure its solubility.

Still more striking proof of the unsaturated nature of hydrogen chloride and nitride is afforded by the manner in which these two gases at once interact when brought into contact, forming solid ammonium chloride, NH_4Cl , a well-defined crystalline substance.

Whilst, therefore, a single atom of carbon combines with four atoms of hydrogen and these four atoms of hydrogen satisfy it and are satisfied by it practically entirely, no one of the other three elements considered is satisfied by the number of hydrogen atoms with which it can combine—in other words, chlorine, oxygen and nitrogen in contradistinction to carbon are all possessed of a greater atom-fixing power, a higher valency, than is indicated by their affinity for hydrogen.

But this higher or extra valency is a function of a different order from the normal or hydride valency, as it may be termed: the latter being a fixed the former a variable and dependent function.

Thus if the hydrogen in hydrogen chloride be displaced by the hydrocarbon radicle methyl, CH_3 , a neutral and practically inert substance, methyl chloride, CH_3Cl , is produced. In like manner both methyl alcohol and methyl ether, CH_3OH and CH_3OCH_3 , are not only far less active substances than water is, but their boiling points are considerably lower than that of water, the alcohol boiling at 66° , the ether being a gas although of higher molecular weight than the alcohol. If methyl be substituted for hydrogen in ammonia, however, it has little influence on the nitrogen in depriving it of its power of attracting acids; the solubility in water of the amine $(\text{CH}_3)_3\text{N}$,

however, is much lower than that of ammonia, whilst the boiling point is much higher. But in the case of aniline, $C_6H_5 \cdot H_2N$, which is derived from ammonia and from phenyl or benzene, C_6H_6 , containing as it does the radicle phenyl, C_6H_5 , in place of one of the atoms of hydrogen of ammonia, the basic tendencies of the nitrogen atom are almost neutralised, aniline being a very weak base in comparison with ammonia. And if the phenyl in aniline be modified or weighted by introducing chlorine atoms in place of several of its hydrogen atoms, the basic properties are still further weakened and eventually entirely neutralised, trichloraniline, $C_6H_2Cl_3 \cdot H_2N$, having no power of combining with acids.

It is not easy, scarcely possible, indeed, to embody facts such as these in models corresponding to that by which carbon is represented and which gives expression so faithfully to its dominant qualities.

In the case of carbon, a tetrahedron with four rods directed from its centre and fixed rigidly at its four terminal points serves to indicate both the number of affinities at the disposal of the carbon atom and the direction in which, apparently, these can act. Hydrogen, like carbon, being also an element of steadfast behaviour, its valency never exceeding unity, may be represented similarly by a ball provided with a single hole into which the rods representing the single combining affinity may be rigidly inserted. Such models serve to emphasise the fact that hydrogen and carbon respectively act invariably as monadic and tetradic elements.

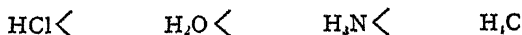
And these are circumstances which it is of importance to emphasise, for if it be the fact that hydrogen is a consistent monad, it necessarily follows that the residual affinity which is manifest in so high a degree in hydrogen chloride, oxide and nitride is in no way resident in the hydrogen but entirely in its associate—that is to say, in chlorine or oxygen or nitrogen; the residual affinity of these elements is of such nature, moreover, that it cannot be satisfied either by hydrogen alone or by any equivalent radicle such as methyl.

To express the peculiar behaviour of oxygen, a ball may be taken through which pass two rods, both free to move in and out. When both rods are drawn out to the maximum extent, the model represents dyad oxygen as it is in a saturated compound; when, however, the rods project partly on both sides

of the sphere, the model is indicative of the tetradic condition of oxygen, the intensity of the secondary or extra valency being indicated by the extent to which the rods project. For example :

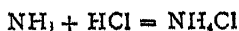


This method of representation is not applicable, however, to chlorine and nitrogen, as these elements both increase in valency to the same extent as oxygen does, that is to say, to the extent of two units, two latent affinities becoming active in both cases when the normal or hydride valency is exceeded. At present we have no explanation to offer of these peculiarities. The formula of the several hydrides considered are best written, therefore, simply as follows :



to indicate the existence of unsaturated affinities and their number in the dominant element in the case of three of the four compounds.

It will be desirable now to consider the nature of the compounds formed by the association of the hydrides of chlorine, oxygen and nitrogen. That formed from hydrogen chloride and ammonia may be taken first, as it is a well-defined stable compound derived from single molecules of the two substances :



The composition of this compound, *ammonium chloride*, has been the subject of much discussion in days gone by. In the first place, it is noteworthy that it resembles most closely in all essential particulars the haloids or metallic halides such as common salt, NaCl, potassium chloride and similar substances—the compound radicle ammonium, NH_4 , playing a part in it exactly similar to that of the simple metallic radicle in these latter. From this it may be argued that when interaction takes place the hydrogen and chlorine in hydrogen chloride part company and combine separately with the nitrogen of ammonia. If this were not the case and the hydrogen atom of the chloride retained its individuality in the new compound, it should be

possible to obtain different derivatives according as this hydrogen atom or one of those in ammonia were displaced by some equivalent radicle such as methyl. Calling the three hydrogen atoms in ammonia abc and that in hydrogen chloride d , if the four atoms of hydrogen $abcd$ in ammonium chloride be of equal value, it should not matter in what order they are displaced. If, however, d be different from abc and different radicles were introduced in place of the four hydrogen atoms at one time in one order and at another time in another, the result would be different. A large number of such substituted ammonium compounds have been prepared, but in no case has any such difference been met with. It is therefore assumed that in ammonium chloride the four hydrogen atoms are of equal value and symmetrically distributed about the nitrogen atom and the associated negative radicle.

In the case of ammonia itself, the functional identity of the three hydrogen atoms has been established beyond question by preparing from it substituted ammonias by introducing three different radicles abc in place of its three hydrogen atoms. Whatever the order in which the radicles are introduced, whether it be



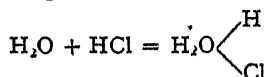
the product eventually obtained is always one and the same substance.

But this conclusion—that when hydrogen chloride and ammonia interact a re-arrangement takes place as the result of the separation and independent association of the elements of the hydrogen chloride molecule with the nitrogen atom of the ammonia molecule—has far-reaching consequences if the argument be extended to the other cases under consideration.

Then it may be asked, What happens when ammonia is brought into contact with water? It is clear that the two compounds interact in some way. Is the behaviour of hydrogen oxide towards ammonia in any way similar to that of hydrogen chloride? It may be supposed that, like this latter, it is divisible into two radicles, H and OH. Is it so divided and do these radicles enter separately into combination with the nitrogen atom, forming ammonium hydroxide, $\text{NH}_4.\text{OH}$? Since ammonium chloride is the analogue of sodium chloride, ammonium hydroxide should be very similar to sodium

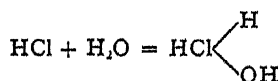
hydroxide or caustic soda, $\text{Na}(\text{OH})$. In point of fact it is so only to a very slight extent. The conclusion is forced upon us, therefore, that a small, but only a very small, proportion of ammonium hydroxide is formed when ammonia dissolves in water; but as dissolution takes place so readily and to such an extent, it is difficult to avoid the conclusion that combination does take place, and it seems not improbable that a compound such as is represented by the formula $\text{H}_3\text{N} = \text{OH}$, is the main product. Apparently, therefore, ammonia has much less influence over hydrogen oxide than it has over hydrogen chloride.

Applying a similar argument in the case of hydrogen chloride and water, it may be supposed that chloride and oxide interact in the following manner:

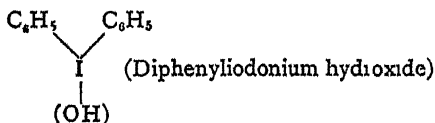


This product, being unstable and having no tendency to crystallise under ordinary conditions, cannot be isolated. But sulphur—which is own brother, as it were, to oxygen among the elements—gives rise to compounds of this type, such as trimethylsulphonium chloride, $\text{S}(\text{CH}_3)_3\text{Cl}$. There is, therefore, every reason to believe that compounds of oxygen may exist such as that postulated above.

The question arises whether the reciprocal interchange is not also effected, namely:



For reasons which cannot well be discussed in this article, it is more than probable that it is. And this conclusion also finds support in the argument from analogy. Thus iodine, which is so closely related to chlorine, is known to give rise to well-defined compounds in which it exercises the functions of a triad, such as—



which, it may be pointed out, is an altogether remarkable compound, inasmuch as it has the properties of a caustic alkali,

the group $(C_6H_5)_3I$ exercising the functions of the atom of sodium in sodium hydroxide. Such are the wonders of chemistry!

To carry the argument one stage further: the compound $(CH_3)_3SCl$, in which sulphur functions as a tetrad, contains only two different radicles. If in place of the three similar hydrocarbon radicles three which are different be associated with a sulphur atom, optically active compounds are produced, as in the case of carbon. In this way proof is obtained that sulphur can simulate carbon: the tetrahedron becomes the model of the sulphur atom as well as of the carbon atom. And since oxygen is very closely related to sulphur, it may be assumed that oxygen also can exercise "tetrahedral" functions and that it may be expected to behave more or less like carbon. In not a few cases it does. Thus a number of compounds are known in which four atoms of carbon are associated with one atom of oxygen in a ring. Such rings are formed with exceptional readiness, so that it would seem to be probable that the directions in which the affinities of the oxygen atom act naturally are not very different from those in which the affinities of carbon act. But such arguments must not be pressed too far; the depth of our inability to account for the peculiar loves and affinities of the atoms must not be forgotten.

EPILOGUE

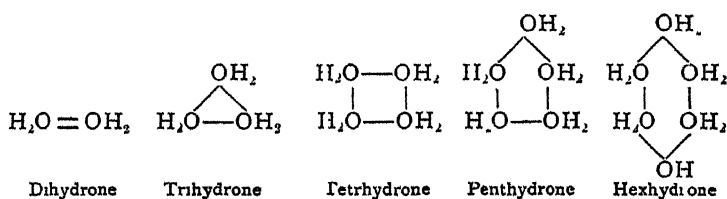
We may now ask, How is liquid water itself formed from hydrogen oxide? What may we suppose the composition and constitution of liquid water to be? Its properties are altogether incompatible with the assumption that it consists only of simple molecules such as are represented by the formula H_2O ; and yet, as steam consists of these, such simple molecules must be present in it, as the maintenance of equilibrium between a liquid and its vapour necessarily involves the constant passage of gaseous molecules from and into the liquid.

To mark the distinction between liquid water and the fundamental molecule represented by the formula OH_2 , from which it is formed, it is desirable to confine the term "water" to the liquid and to term the fundamental molecule *Hydrone*.¹

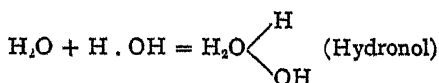
Hydrone is comparable with the hydrocarbon radicle methylene, CH_2 , which, however, cannot exist *per se*, its affinity

¹ Compare *Royal Society Proceedings*, 1908, A, vol. 81, 80.

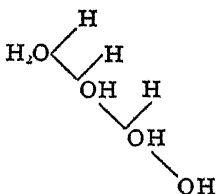
for itself being of such intensity that when freed from other radicles it at once polymerises, giving rise to dimethylene or ethene (ethylene), $H_2C=CH_2$. It is to be supposed that hydrone behaves in a similar manner: that is to say, that when it becomes liquid water it gives rise to a series of polyhydrones corresponding to the polymethylenes, thus:



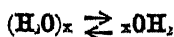
In addition, interaction probably takes place in the manner pictured by the expression



And it is both conceivable and indeed probable that still longer hydronol chains are also formed, such as



In short, the picture to be formed of the state of water is that of a complex mixture of molecules of the general composition $(OH)_x$, x having many values, the proportion in which various kinds of molecules are present being determined by the external conditions (temperature and pressure). But although quiet in appearance, the internal condition of the liquid must be supposed to be one of unceasing turmoil, owing to the perpetual passage backwards and forwards of the various molecules from one state of complexity into another, as summarised in the expression



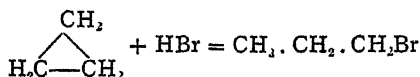
As to the activities of the various molecules, there must be great differences between them. The hydrone molecules

are undoubtedly the most active; and next to these come the molecules of hydronol: the increase in the activity of water as its temperature is raised may, in fact, be ascribed to an increase in the proportion of hydrone and hydronol in the liquid. As to the activity of the polyhydrones, it is possible to form an opinion only on grounds of analogy.

In the case of the polymethylenes, the activity diminishes from ethylene to pentamethylene. Thus ethylene combines with the greatest readiness with bromine and with hydrogen bromide:

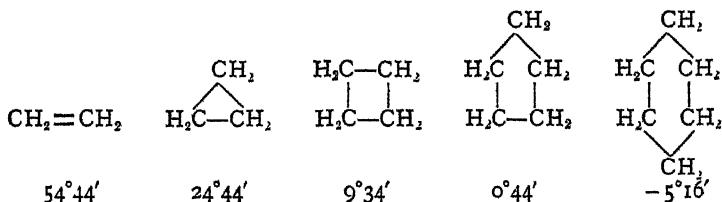


The higher polymethylene rings remain unbroken when submitted to the action of bromine; but the trimethylene ring is without difficulty opened out by means of hydrogen bromide:



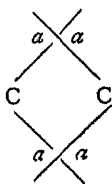
The tetramethylene ring is somewhat less readily ruptured and the penta- and hexa-methylene rings are unaffected by hydrogen bromide.

Assuming that the configuration of the carbon atom is tetrahedral and that the affinities act in the manner pointed out above, the angle at which two affinities meet is $109^\circ 28'$. To account for the behaviour of the different polymethylenes, von Baeyer has argued that if it be supposed that in the formation of ethylene two affinities of the one carbon atom become united with two affinities of the other carbon atom, the affinities would be forced from their normal angular positions into parallelism—that is to say, through an angle of $\frac{109^\circ 28'}{2} = 54^\circ 24'$; but in the case of the higher polymethylenes the distortion would be less—thus:



The general behaviour of the hydrocarbons under consideration, however, is scarcely that of strained molecules, but rather

that of molecules endowed with various degrees of attractive power. If von Baeyer's argument be so modified that it be supposed that the affinities, instead of being deflected, act practically at their proper angle and more nearly neutralise one another the more nearly they come into parallelism, the angle α at which the affinities meet may be taken as a measure of the free affinity latent in the molecule; and the larger the angle the greater will be the activity of the compound. Ethylene (dimethylene) from this point of view



is an eminently unsaturated compound; whilst pentamethylene is all but saturated, as the several pairs of affinities nearly overlap,¹ the angle α being reduced almost to nothing.

Assuming, for the reasons already stated, that the tendency of oxygen is to simulate carbon, it should follow that the activity of the polyhydrone molecules in water diminishes progressively as they increase in complexity: consequently that penthydrone is practically inactive.

If water consist on the one hand of simple molecules which have a strong tendency to cling together, and on the other of relatively heavy inert molecules, such as those of penthydrone (OH_2)₅, it is easy to understand why it boils at such an elevated temperature as 100°.

A point is now reached at which the story becomes exciting it is therefore time to write, "To be continued in our next."

In conclusion it may be asked, Can water be the subject of an idyl? An idyl has been defined as a poem which has for

¹ This argument will be best understood if a graphic construction be resorted to. For this purpose a number of V-shaped strips, cut to an angle of 109°28', should be made out of cardboard. These are conveniently made three-quarters of an inch wide and the two limbs may be each 6 inches long. Lines should be ruled down the middle of each limb; the cardboard should be perforated 3 inches distant from the point of intersection of the two lines on each strip. By joining such strips together at the perforated points in twos, threes, fours, fives and sixes by means of drawing pins placed head down on the table and then fixing the joint by covering the points of the pins with discs of cork, models are obtained which represent the various systems.

its object a vivid and simple representation of ordinary objects in pastoral nature or of scenes or events of pastoral life. Tennyson, however, has written the *Idyls of the King*. Kings are scarcely common objects of the seashore, whilst water certainly is prominent among these and a true king among earthly materials: therefore it may well form the subject of an idyl, and this story may be added to the many other marvellous tales that are told of its mysterious ways and wondrous doings.

Being thus brought into Wonderland, we may well recall the classical assertion of the Duchess, "'Tis love, 'tis love, that makes the world go round." Had she lived in these days, having studied physiography at school and college, she would have been aware perhaps that water is the primary moving spirit on our globe; that even love is not possible without water—indeed, that the part which water plays in conditioning life and in the maintenance of life is such that its importance cannot be exaggerated.

Yet it is daily and hourly used and wasted entirely without thought!

The poetry of moisture must some day take its place along with that of motion—when all see that the earth gives forth her fruits in due season and in due measure only when fair hydrone, descending from the air above, clasps her in warm and beneficent embrace.

PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES

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III

EARLY PLEISTOCENE MAN AND THE TASMANIANS

To commence a chapter on pleistocene man by an account of a recent race might well seem a wilful anachronism; the Tasmanians, however, though recent, were at the same time a palæolithic or perhaps even eolithic race; and they thus afford us an opportunity of interpreting the past by the present—a saving procedure in a subject where fantasy is only too likely to play a leading part. We will therefore first direct our attention to the habits and mode of life of this isolated people, the most unprogressive in the world, which at the close of the nineteenth century was still living in the dawn of the palæolithic epoch.

As regards clothing, the Tasmanians dispensed with it. They habitually went about in a state of nakedness, except in winter, when the skins of kangaroos were sometimes worn. To protect themselves from rain they daubed themselves over with a mixture of grease and ochre. Yet they were not without their refinements: the women adorned themselves with chaplets of flowers or bright berries, and with fillets of wallaby or kangaroo skin, worn sometimes under the knee, sometimes around the wrist or ankle; the young men were also careful of their personal appearance—a fully dressed young man wore a necklace of spiral shells and a number of kangaroos' teeth fastened in his woolly hair.

They paid great attention to their hair; it was cut a lock at a time with the aid of two stones, one placed underneath as a chopping-block, the other being used as a chopper. A sort of pomatum made of fat and ochre was used as a dressing.

The Tasmanians had no houses, nor any fixed abode; they

wandered perpetually from place to place in search of food, and their only protection from wind and weather, in a climate sometimes bitingly cold, was a rude screen made by fixing up strips of bark against wooden stakes.

Their implements were few and simple, made of wood or stone; their weapons, whether for the chase or war, were of wood. Of these the spear was the most important; it was fashioned out of the shoots of the "ti" tree, which are distinguished for their straightness. To convert one of these into a spear was an operation demanding considerable skill and care: the stick was first warmed over a fire to render it limber, and if not quite straight was corrected by bending with both hands while held firmly between the teeth. Thus the human jaw was the earliest "arrow-straightener." The end was hardened by charring in the fire, and sharpened by scraping with a notched flake of stone. With a similar implement the bark was removed and the surface rendered round and smooth. When finished it was a formidable weapon; a good spear balanced in the hand as nicely as a fishing-rod; it could be hurled for a distance of sixty yards with sufficient force to pass through the body of a man. The aim of the Tasmanian was good up to forty yards. To keep their spears in good condition, when not in use, they were tied up against the trunk of a tree, selected for its straightness.

The only other weapon was the club or waddy, about two feet in length, notched or roughened at one end to give a grip, and sometimes knobbed at the other; the shaft was scraped smooth like the spear. Its range was over forty yards.

The stone implements, which served a variety of purposes, were made by striking off chips from one flake with another: in this occupation a man would sit absorbed for hours at a time. Flint is not known in Tasmania, and a fine-grained sandstone or "phthanite" served as a substitute; it is not so tractable as flint, however, and this may partly account for the inferior finish of much of the Tasmanian workmanship.

A double interest attaches to the notched stone or "spoke-shave," used for scraping the spear; the spear itself is perishable, for wood soon decays, and no wooden implements are known to have survived the palæolithic period; but the stone spoke-shave, which implies the spear, and in its smaller forms the arrow, may endure for an indefinite time. Many excellent

examples of such implements are known under the name of hollow scrapers or "raclours en coches," both from palæolithic and neolithic deposits.

A large rough tool, delusively similar to the head of an axe, was made by striking off from a block of stone a thick flake with a single blow, and dressing the side opposite the surface of fracture by several blows directed more or less parallel to its length. This is not unlike the ancient palæolithic implement which the French call a "coup de poing" and the Germans a "Beil," *i.e.* an axe. In English it has no name, though it was at one time very inappropriately spoken of as a celt, a term never used now in this sense. Anthropologists are generally agreed that the palæolithic "coup de poing" was not provided with a haft, but was held directly in the hand; and that it was not used simply as a "chopper": it is extremely gratifying therefore to find that the Tasmanians had no notion of hafting their homologue of the "coup de poing," and that it served a variety of purposes, among others as an aid in climbing trees. It was the women who were the great climbers: provided with a grass rope, which was looped round the tree and held firmly in the left hand, they would cut a notch with the chipped stone¹ and hitch the great toe into it; then adjusting the rope they would cut another notch as high, it is said, as they could reach; again hitch themselves up, and so on till they attained the requisite height—sometimes as much as 200 feet. In this way they pursued the "opossum" up the smooth trunk of the gum-tree. Many stories are told of their expertness: on one occasion a party of lively girls chased by sailors made a sudden and mysterious disappearance; on looking round a number of laughing faces were descried among the branches of the trees, into which the girls had swarmed in the twinkling of an eye.

There is great inconvenience in having no special name for this kind of implement—greater perhaps than attaches to the introduction of a new word; I propose therefore to use "boucher" as an equivalent of the "coup de poing," thus honouring the memory of Boucher des Perthes, who was the first to compel the attention of the scientific world to these relics of the past. This kind of nomenclature has already been

¹ Prof. Tylor describes this as a quoit-like stone, 4 to 6 in. across, and chipped about two-thirds round the edge: *Journ. Anthr. Inst.* 1893, vol. xxiii. p. 142.

introduced by physicists, as for instance in the terms volt, joule, watt, and others.

Another implement was an anvil, formed of a plate of stone chipped all round into a circle, about 7 in. in diameter, 1½ in. thick in the middle, and 1 in. thick at the edge. On this the women broke the bones left after a meal to extract the marrow, using another stone, about 6 in. in diameter, as a hammer. M. Rutot has described several such anvils (*enclumes*), but of a ruder make, from early palæolithic deposits.

One of the commonest tools was the scraper, a flake of about 2 in. in diameter, carefully dressed by chipping on one side only to a somewhat blunt edge. The edge was not serrated, and great skill was required to keep the line of flaking straight: it was used for flaying animals caught in the chase, and as well, no doubt, for other purposes. To test its powers Prof. Tylor sent a specimen to the slaughter-house requesting the butcher to try his skill in flaying with it. The notion was rather scornfully received, but on trial the flake was found to be admirably adapted to the task, removing the skin without damaging it by accidental cuts.

The country seems to have afforded the Tasmanians a fair amount of game. Kangaroos, wallaby, "opossums," bandicoots, the kangaroo rat, and the wombat were all excellent eating, especially as cooked by the natives. The animals were roasted whole in the skin and cut up with stone knives; the ashes of the wood fire were sometimes used as a seasoning in default of salt. Cooking by boiling was unknown to them, and when introduced to their notice they expressed their disapproval of a method which produced such unpalatable results.

There were several kinds of birds, such as the emu, now extinct in Tasmania, black swans, mutton birds, and penguins. The eggs of birds were also eaten. Snakes and lizards were put under contribution, as well as grubs extracted from hollow trees, and said by Europeans to be dainty morsels, with a nutty flavour reminiscent of almonds.

Fish the Tasmanians did not eat, simply because they were ignorant of the art of fishing, nets and fish-hooks being unknown to them; but cray-fish and shell-fish were an important article of diet. The women obtained the shell-fish by diving, using a wooden chisel, made smooth by scraping with a shell, to displace those which live adherent to the rocks.

The shell-fish were roasted; and the empty shells, thrown away near the hearths, grew into enormous mounds or kitchen middens, which still afford interesting material to the anthropologist. Most of the shells found in these belong to genera which are universally eaten by mankind, such as oysters, mussels, cockles, limpets, periwinkles (*Turbo* and *Purpura*), and ear-shells (*Haliotis*). The periwinkles were broken by a stone hammer on a stone anvil, and these implements, as well as stone knives, are also found in the kitchen middens.

Several kinds of plants furnished them with vegetable food—the young shoots of ferns, roots of bulrush, the ripe fruit of the kangaroo apple (*Solanum laciniatum*), a fungus with a truffle-like growth, and sea-wrack. These were cooked by broiling.

Water was their usual but not their only drink, for they well understood the virtues of fermented liquor. A species of gum-tree (*Eucalyptus resinifera*) yields when tapped a slightly sweet juice, resembling treacle; this they allowed to collect in a hole at the bottom of the trunk, where it underwent a natural fermentation, and furnished a kind of coarse wine.

Fire was obtained either by the simple plan of rubbing the pointed end of a stick to and fro in a groove cut in another piece of wood or by the drill method, *i.e.* by rotating one stick in a hole sunk in another. Each family kindled its own fire at its own hearth, the hearths being separated by intervals of fourteen to twenty yards.

The following statement of Backhouse is of interest in connection with the discovery of marked stones in some European caves. He writes: "One day we noticed a woman arranging stones that were flat, oval, and about two inches wide, and marked in various directions with black and red lines. These we learned represented absent friends, and one larger than the rest a corpulent woman on Flinders Island, known as Mother Brown."

It is said that rude attempts were sometimes made to represent natural objects by drawings. Very poor sketches of cattle, kangaroo, and dogs done in charcoal are mentioned; but cattle and dogs suggest the possibility of European influence. The fact that large pieces of bark have been found with crudely marked characters like the gashes the natives cut in their arms is of more importance.

The facts we have thus briefly summarised include almost

all that I can discover bearing directly on our subject. For the sake of completeness it may be as well to give some account of the bodily characters of this interesting people, and a few words as to their history.

The Tasmanians were of medium stature, the average height of the men being 1,661 mm., with a range of from 1,548 to 1,732 mm.; the average height of the women was 1,503 mm., with a range of from 1,295 to 1,630 mm. The colour of the skin was almost black, inclining to brown. The eyes were small and deep-set, beneath strong overhanging brows; the nose short and broad, with widely distended nostrils; the mouth big; and the teeth larger, it is said, than those of any other existing race.

The hair was black and grew in close corkscrew ringlets. The men had hair on their face—whiskers, moustache, and beard, and on the borders of the whiskers it assumed the form of tufted pellets like peppercorns.

It is a commonplace amongst biologists that characters of apparently the most trivial significance are precisely those which are of the greatest value as a means to classification, and it is on the degree of curliness or twist in the hair that the most fundamental subdivision of the human race is based. We thus recognise three groups: one in which the hair is without any twist—that is, perfectly straight—the *Lissentrichi*; another in which it is twisted to an extreme, as in the Negro or Bushman—the *Ulotrichi*; and a third in which the hair is only twisted enough to be wavy, as in many Europeans—the *Cymentrichi*. The Tasmanian is *ulotrichous*, like the Negro and most other races with very dark skins.

The bony framework, being more resistant to decay than the rest of the body, is more likely to be preserved in the fossil state, and has therefore a certain amount of importance in our study. We shall restrict our description, however, to the skull, as more is to be learnt from this than from any other portion of the skeleton.

The skull of the Tasmanian is of a characteristic form, so that a practised eye can readily distinguish it from that of other races. Looked upon directly from above, its outline is oval or more or less pentagonal; its greatest breadth lies considerably behind the middle line. The crown rises into a low keel, bordered by a groove-like depression on each side; the

sides of the skull are rounded and swell into large parietal bosses.

It is neither long nor short (mesaticephalic), and the ratio of its breadth to its length (cephalic index) is 73·9, as determined from measurement of twenty-six examples. The cranial capacity is the lowest yet met with among recent races, measuring on the average 1199 cc.; in the men the average rises to 1306 cc., in the women it falls to 1093 cc.

The face is remarkably short, and presents a peculiarly brutal appearance; the brow-ridges and glabella are strongly marked, and there is a deep notch at the root of the nose. The jaws project, but not to the extreme degree which is characteristic of the Negro, nor even so much as in some Australians. The lower jaw is small, disproportionately so when compared with the teeth, which, as already observed, are very large. In consequence of this misfit the natives suffered grievously from dental troubles.

In endeavouring to discover the people to whom the Tasmanians are most closely related, we shall naturally restrict our inquiries to the Ulotrichi, for, as we have seen, the Tasmanians belong to this group. Huxley thought they showed some resemblance to the inhabitants of New Caledonia and the Andaman Islands, but Flower was disposed to bring them into closer connection with the Papuans or Melanesians. The leading anthropologists in France do not accept either of these views. Topinard states that there is no close alliance between the New Caledonians and the Tasmanians, while Quatrefages and Hamy remark that "from whatever point of view we look at it, the Tasmanian race presents special characters, so that it is quite impossible to discover any well-defined affinities with any other existing race," and this probably represents the prevailing opinion of the present day.

The Tasmanians appear to have been an autochthonous people, native to the soil, the surviving descendants of a primitive race, elsewhere extinct or merged into a preponderant alien population. Frequenting the coast, and yet destitute of sea-going craft, it is scarcely likely that they reached Tasmania from any of the outlying islands; and it is far more probable, as our foremost authorities now maintain, that they crossed over from Australia at a time antecedent to the formation of

Bass Strait, when the Australian cordillera was continuous from Tasmania to New Guinea.¹

The primitive ancestors of the race may have been widely distributed over the Old World : displaced almost everywhere by superior races, they at length became confined to Australia and Tasmania, and from Australia they were finally driven by the existing aborigines of that continent, who were prevented from following them into Tasmania, because by that time the Australian cordillera had broken down, the bridge between the two territories had collapsed by subsidence, and Bass Strait barred the passage to the encroaching foe.

When the more civilised nations of the north had succeeded in subjugating the sea to their enterprise, even the ocean itself failed in its protection to this unfortunate people, and with the arrival of English colonists its doom was sealed. Only in rare instances can a race of hunters contrive to co-exist with an agricultural people. When the hunting ground of a tribe is restricted owing to its partial occupation by the new arrivals, the tribe affected is compelled to infringe on the boundaries of its neighbours : this is to break the most sacred "law of the Jungle," and inevitably leads to war : the pressure on one boundary is propagated to the next, the ancient state of equilibrium is profoundly disturbed and inter-tribal feuds become increasingly frequent. A bitter feeling is naturally aroused against the original offenders, the alien colonists : misunderstandings of all kinds inevitably arise, and are only too likely to lead to bloodshed, and a conflict between natives and colonists, in which the former, already weakened by disagreements among themselves, must soon succumb. So it was in Tasmania.

The estimates which have been given of the number of the population at the time Europeans first became acquainted with the country differ widely : the highest is 20,000, but this is probably far in excess of the truth. After the war of 1825 to 1831 there remained scarcely 200. These wretched survivors were gathered together into a settlement, and from 1834 onwards every effort was made for their welfare, but "the white man's civilisation proved scarcely less fatal than the

¹ The distinction between the Australian and Oriental faunas renders it impossible to maintain this view. Man must have had special means by which he could enter Australia unaccompanied by other animals ; rafts or canoes seem indicated.

white man's bullet," and in 1877, with the death of Truganini, the last survivor, the race became extinct.

It is a sad story, and we can only hope that the replacement of a people with a cranial capacity of about 1,200 c.c. by one with a capacity one-third greater may prove ultimately of advantage in the evolution of mankind.

The world certainly needs all the brains it can get: at the same time it is not very flattering to our own powers of intelligence to find that we allowed this supremely interesting people, the last representatives of one of the earliest stages of human culture, to perish without having meanwhile ascertained all that could be known about it. What we do know is very little indeed; a book of about three hundred pages contains almost every scrap of trustworthy information.¹

If any other nation had shown the same disregard for a human document of such priceless value, we should be inclined to discredit its civilisation. Even now, in this twentieth century, it cannot be said that the British Government takes such an intelligent interest in the numerous primitive peoples which it has taken into its charge as we have a right to expect, at least from a State having any regard for the advancement of learning.

The first to call attention to the resemblance between the stone implements of the Tasmanians and those of palæolithic man was Prof. E. B. Tylor.² Subsequently Mr. R. M. Johnston³ compared them with the "eoliths" figured by Ribiero already alluded to (SCIENCE PROGRESS, 1908, p. 348). Prof. Tylor⁴ has repeatedly returned to the subject; and in 1905 exhibited specimens before the Archæological Institute, when he made the following statement: "I am now able to select and exhibit to the Institute from among the flint implements and flakes from the cave of Le Moustier, in Dordogne, specimens corresponding in make with such curious exactness to those of the Tasmanian

¹ H. Ling Roth, *The Aborigines of Tasmania*, Halifax, England, 1899.

² E. B. Tylor, *The Early History of Mankind*, London, 1865, p. 195.

³ R. M. Johnston, *Systematic Account of the Geology of Tasmania*, 1888, p. 334.

⁴ E. B. Tylor in Preface to H. Ling Roth, *The Aborigines of Tasmania*, 1st Edition, 1890; 2nd Edition, 1899. On the Tasmanians as Representatives of Palæolithic Man, *Journ. Anthr. Inst.* 1893, xxiii. pp. 141-152, 2 pls. On the Survival of Palæolithic Conditions in Australia and Tasmania, *Journ. Anthr. Inst.* 1898, xxviii. p. 199. On Stone Implements from Tasmania, *Journ. Anthr. Inst.* 1900, xxx. p. 257.

natives, that were it not for the different stone they are chipped from, it would be hardly possible to distinguish them.”¹

Since then Prof. Tylor has been led to believe that an even closer resemblance can be traced between the so-called plateau implements and the Tasmanian. This is a view which has also recommended itself to M. Rutot.² If this could be established it would invest the Tasmanian implements with peculiar interest.

The plateau “implements” are so called because they are found in gravels capping the high plateaux of Kent and elsewhere. They were first discovered by Mr. B. Harrison, of Ightham, who brought them before the notice of Sir Joseph Prestwich; and this observer, famous for the caution and sagacity of his judgment, expressed in unqualified terms his conviction that they showed signs of the handiwork of man. Sir John Evans, a fellow-worker with Prestwich, and equally distinguished for his acumen and insight, was unable, however, to share this opinion, and at present the question is involved in the raging vortex of the “eolith” controversy.

The plateau gravels are no doubt very ancient; they lie at a higher level than any of the existing river terraces, and cannot be referred to any of the existing river systems. Prestwich spoke of them as glacial or pre-glacial; M. Rutot assigns them to the Pliocene.

The question as regards the “implements” is an extremely difficult one. A great number of the Tasmanian forms are so rude and uncouth that, taken alone, we should have little reason to suspect that they had been chipped by man; some, on the other hand, show signs of skilful working, and leave us in no doubt. It is on these last that our judgment should be based in a study of the Tasmanian art. As to the rest, “*noscitur e sociis*.” They are distinguished by two very definite characters. In the first place their fundamental form is that of a flake which has been split off from a larger fragment. They never commence their existence as fragments already existing in a natural state. And next, the finer dressing of the stone is always confined to one face; if a boucher, there is one face obtained by a single blow which detached it from the parent mass, and an opposite face with secondary flaking; if a scraper,

¹ *Journ. Anthr. Inst.* 1895, vol. xxiv. p. 336.

² A. Rutot, “La Fin de la Question des Éolithes,” *Bull. Soc. Géol. Belg.* 1907, xxi. p. 211.

the marginal dressing is produced by the removal of chips always struck off in the same direction, as in some neolithic scrapers.

If we judge the Tasmanian implements by the best examples, we should in fairness extend the same treatment to the plateau "implements." The best of these do indeed show some superficial resemblance to the Tasmanian, especially in general form, and this is particularly true of the hollow scrapers. In connection with these we may cite the following statement made by Prestwich when speaking of the plateau implements. He says: "A very common form is a scraper in the shape of a crook, sometimes single, sometimes double, such as might have been used *for scraping round surfaces like bones or sticks.*" The part we have placed in italics shows remarkable insight.

But the comparison is scarcely sustained when we enter into a minute investigation. To begin with, the fundamental form of the plateau "implement" is rarely—so far as I know, never—artificial. On the hypothesis that these fragments were used by man, we must suppose that, to begin with, he simply selected such bits of flints, lying scattered about, as he thought would serve his ends. In the next place, the chipping at the edges, though certainly confined to one side, has not the closeness nor regularity that distinguishes the best Tasmanian scrapers, which, as already remarked, sometimes suggest neolithic rather than palæolithic workmanship. The confused and clumsy chipping of the plateau "hollow-scraper" does not produce an efficient edge, and it seems hard to believe that a being with sufficient intelligence to conceive the idea of a spoke-shave should not have succeeded in making a better one.

Mr. Henry Balfour, one of the first to study Tasmanian implements and to recognise their palæolithic affinities, regards them as representing a separate cult. At the same time he is willing to admit that, in the doubtful event of the plateau flints proving to be true implements, it is to these rather than to palæolithic types that he would refer them. The plateau "implement," however, is not the only one of its kind; there are others of later date, but antecedent to the beginning of the palæolithic age. These are the Reutelian, Mafflian, and Mesvinian of M. Rutot. The relations of the Mafflian and Mesvinian to the palæolithic can be plainly seen in the fine section at Helin near Spiennes. The Reutelian occurs else-

where. I am personally much indebted to M. Rutot for the pains he bestowed on me some two years ago, when he guided me to the principal sections, and demonstrated the characters of the supposed implements in his laboratory. The logic of his method and the excellence of his examples¹ at that time nearly persuaded me; I have since oscillated repeatedly between a positive and negative conclusion; but now, after a closer study, I feel convinced that the balance of probabilities is largely in favour of the artefact origin of some at least of these forms of flint. I have now before me several flakes, taken with my own hand from the Mesvinian horizon, which all present essentially the same shape and characters. One side is formed by a single conchoidal surface, the other by two or three faces which run parallel with one another, and with the opposite face. If these flakes had been formed by random blows, such as occur when pebbles are hurried along by a river, we should find cones of percussion scattered at random over the surface; but, as a matter of fact, no such cones are anywhere to be seen, even with the assistance of a strong lens, except at the butt end. There we perceive indications of four, or sometimes five, cones of percussion; one of them is given by the bulb of percussion which corresponds with the single conchoidal surface, two with negative bulbs of percussion which are associated with the faces of the opposite side; the remaining cones have not penetrated far into the stone, but they record a blow which must have been struck in approximately the same direction as those which resulted in flaking. But four or five blows delivered in the same direction over a very restricted area of the flake can scarcely be attributed to chance; and when we find the same phenomenon repeated in a great number of flakes all having much the same form, we begin to feel that the blows recorded by the cones of percussion are the result not of accident but intent. They were delivered by an intelligent being, who immediately preceded early palæolithic man.

If we accept this conclusion for the Mesvinian,² we cannot

¹ A. Rutot, "Causeries sur les industries de la pierre," *Rev. de l'École d'Anthropologie de Paris*, 1907, viii. pp. 283-294.

² It is now admitted by M. Hugo Obermaier, who identifies the Mesvinian with the oldest known palæolithic horizon, which he names "früh-Chellean." H. Obermaier, "Die Steingeräte des französischen altpaläolithikums," *Mitth. d. prähistorischen Kom. d. K. Ak. Wiss. Wien*. Bd. ii. No. 1, 1908, pp. 41-125.

exclude the Reutelian implements ; but among these we discover hollow scrapers, resembling the Tasmanian spoke-shaves, and still sharp enough to serve their purpose fairly well. It would therefore seem probable that Reutelian man was in possession of a wooden spear.

The resemblance of some of the Reutelian implements of the Pleistocene epoch to the Tasmanian is certainly very great. Differences no doubt exist ; but reviewing the evidence as a whole it seems sufficient to show that, prior to the advent of Chellean man, Europe was overrun by races whose state of culture was not far removed from that of the now extinct Tasmanians ; as to the physical characters of these races we know absolutely nothing.

Evidently the last word has not been said on this question. Opinions are still at variance, and we must await some fortunate discovery as the final arbiter.

IV

LOWER PALÆOLITHIC MAN

If, suddenly transported to the beginning of the palæolithic epoch, we could survey the face of the earth as it then appeared, we might be surprised at first by its strange and unfamiliar aspect ; but on closer inspection, as we traced one by one its leading features and identified the several continents and seas, we should perceive that the general plan remained the same and that the details alone were changed.

These details, however, were neither few nor unimportant. The whole continent of Europe had enlarged its bounds, and the Atlantic broke against a shore lying far to the west of the British Isles, along a line where soundings now show a depth of 100 fathoms. It looks as though the ocean had sunk 600 feet. The Irish Sea, the English Channel, and the German Ocean, thus deserted, formed wide valley plains, watered by many noble rivers. The Rhine, with its tributaries the Elbe and the Thames, swept in wide meanders to the north till it opened into the sea not far south of the Faeroe Isles ; the Seine, gathering the waters of the south of England and north of France in its flow, continued its course through the fertile plains of the English Channel till it entered the Atlantic a hundred miles west of the farthest point of Brittany

or Cornwall; and the deepest parts of the Irish Sea formed great fresh-water lakes stocked with ancestral salmon.

In the south we might look in vain for the Adriatic, and in place of the Mediterranean we should discover two restricted inland seas, separated by a broad isthmus, which extended from Northern Africa, through Sicily, into Southern Europe.

On the extreme east, Asia was probably united with America, across Bering Strait, by a tract of land which extended an unknown distance to the south, perhaps completing the arc of the Aleutian Islands, now represented on the map by a mere dotted line.

On the extreme west and north an ancient bridge, afterwards to break up into Iceland and the Faeroes, was possibly still standing, and united Europe with Greenland and the east of North America; but this is an open question, to which we shall refer later.

In some places, on the other hand, the sea penetrated farther into the land, as where the Arctic Ocean covered all the region of the gulf of the Obi.

A traveller starting in this ancient world from the banks of the Thames could have made his way over the watershed formed by the Straits of Dover into France, and so through Italy and across Sicily into Africa, which would have then lain open to him from end to end. If instead of entering Africa he had turned to the left, he could have reached India by devious paths, the Malay peninsula, and the East Indies, which, unite here and there by land-connections, would have taken him, with the help of a frail canoe, into Australia, whence he might have wandered into Tasmania.

If he wished to visit North America he would have had, perhaps, a choice of routes, either by the Icelandic bridge or the Alaskan isthmus.

Even before leaving England he would see strange sights by the way: great herds of elephants of an ancient kind (*Elephas antiquus*), the mightier predecessors, perhaps ancestors, of the mighty African elephant; he might witness, not without awe, the infuriated rush of the soft-nosed rhinoceros (*Rhinoceros leptorhinus* or *Merckii*), which bore a horn sometimes as much as three feet in length; disporting itself in the rivers was that shy behemoth the hippopotamus, the mother animal swimming with her young upon her back; sometimes he might catch sight

of the great sabre-toothed lion, *Machairodus*, making its stealthy spring, or hanging, with its great overgrown canines, on to the flanks of a strayed elephant. If he waited by the water places he would be able to watch herds of bison, wild horses, and various kinds of deer, the Irish elk among them, as they came to drink.

A delightfully warm climate might tempt the traveller to make his bed in the open, but, in any case, he would do well to beware before accepting the shelter of a cavern, for there he might encounter the terrible cave-bear, larger than any existing species, or an animal even still more terrible, no other than man himself.

Unfortunately we have no time-machine by which we might revisit these scenes; we must content ourselves by laboriously piecing together the evidence, still more laboriously obtained, which lies sparsely scattered in the gravel of river terraces or in the débris of ancient caves, which is fragmentary at the best and consequently too often full of apparent contradictions. He who attempts to construct a consistent story will sometimes wonder whether he may not be weaving a rope of sand. Classifications are made only to be unmade, and as finer and finer subdivisions are proposed, so our difficulties seem only to increase. Among much that is confused, certain facts, however, seem to stand forth in broad outline, and it is to these that we will direct our attention.

The palæolithic series may be provisionally divided into an upper and a lower group. This proceeding will at all events provide us with useful general terms. These groups may be further subdivided into stages as follows:

Upper Palæolithic	{	Azilian stage	
		Magdalenian stage	
		Solutrian	„
Lower Palæolithic	{	Mousterian	„
		Acheulean	„
		Chellean	„
		Strepyian	„

Some anthropologists, amongst them M. Marcellin Boule and M. Rutot, draw the line between the upper and lower palæolithic below the Mousterian, and they further subdivide the Acheulean into two sub-stages, which M. Rutot names Acheulean I. and Acheulean II. On the other hand, there are

some, like Prof. M. Hoernes, who reject the Strepyian, and recognise no distinction between the remaining three stages of the lower palæolithic, throwing them all together into a single subdivision termed the Chelleo-Mousterian. Inconvenience will naturally attend both an excess and defect of refinement in classification; but to underestimate the value of distinctions is far less likely to elicit the truth, and almost certain to give more trouble in the end.¹

The separate existence of the first three stages, the Strepyian, Chellean, and Acheulean, as well as the true order of their succession in time, is proved in the clearest manner by the classic section St. Acheul and that at Helin, near Spiennes, already alluded to. This latter may also serve to give us some idea of the interval of time between them; and, judging from the thickness of the intervening beds, the Acheulean is separated from the Chellean by a period of approximately the same duration as that which separates the Chellean from the Strepyian.

The Strepyian Stage

In this stage we encounter for the first time implements which, beyond dispute, have been shaped for definite ends.

The distinctive character of the Strepyian industry, according to M. Rutot, is that all the implements retain a considerable part of the original crust of the flint nodule from which they have been fashioned.

The most characteristic is a remarkable weapon evidently intended to be used as a dagger.² An elongated flint nodule,

¹ M. Commont has completely justified M. Mortillet's classification. *Vide* Commont, "Contribution à l'Étude des Silex taillés de St. Acheul et de Moustier," *Bull. Soc. Linn. du Nord de la France*, xvii., 1905, p. 292, xix. 1907. The latest classification of the lower palæolithic by M. Hugo Obermaier is as follows:

- MOUSTERIAN
- ACHEULEAN UPPER
 - Levallois
 - La Micoque
 - „ LOWER (base of La Micoque and Le Moustier)
- CHELLEAN UPPER, with bouchers
- „ LOWER, without bouchers

H. Obermaier, *Mitth. d. prahistorischen Kom. d. K. Ak. Wiss. Wien*. Bd. ii. No. 1, 1908. M. Obermaier correlates the Strepyian of Belgium with the Upper Chellean of St. Acheul.

² M. Hugo Obermaier has cast doubts on the genuineness of these daggers, (H. Obermaier, *op. cit.* p. 89). M. Rutot has replied to M. Obermaier's objections in a pamphlet entitled "The Terrible Secret," 1908, printed by Hayez, Brussels.

such as commonly occurs in the adjacent chalk, has been cleverly chipped into a blade-like end, while the part left unworked furnishes an excellent haft. The black flint exposed at the worked surface is in striking contrast to the opaque, dead white crust which remains on the haft, and presents an astonishingly fresh appearance without patination. In some cases a natural swelling of the nodule occurs just below the blade, and has been fancifully compared to a guard.

There are some forms which anticipate the boucher; they may be likened to a dagger with a very broad blade, or to an almond-shaped nodule trimmed to an edge on each side of one extremity.

Coarse examples of scrapers (*grattoirs*) and chisels (*racloirs*) are not uncommon, and recall certain "eolithic" forms. M. Rutot makes a good point in calling attention to the association of these with the more finished implements, as an argument in favour of the artefact character of the earlier eoliths. There is apparently a transition from preceding stages of culture characterised by eoliths only, into the Strepyian with eoliths and daggers; and if in still later deposits skilfully made weapons alone were found, to the exclusion of eoliths, the fact would be significant. But this is not the case; the eolith, whether made by nature or by man, persists to the end. I have seen it myself in ancient settlements of the Bushmen, and the only reason I had for supposing it to have been used by man was the fact of its association with other signs of Bushmen influence.

The fauna of the Strepyian is very imperfectly known.

The Chellean Stage

The distinctive Chellean implement is the boucher. It is usually of large size, not unlike in size and form two hands apposed palm to palm, but it may attain a length of ten inches, or it may not exceed two or three inches. Most commonly it is somewhat almond-shaped, sometimes it is more triangular, and rarely oblong with rounded ends. Not uncommonly it retains a part of the surface of the original nodule or pebble from which it has been shaped.

It has been dressed by coarse flaking on both sides, and the flaking of opposite sides meets along the edge in a wavy line, so that this edge, which was used for cutting or scraping, is characterised by its irregularity.

As we have already seen, it was used in all probability without the intervention of a helve. At the same time, too much stress should not be laid on analogy with the Tasmanian implement, for that was used in a very different environment, destitute of ferocious carnivora and of colossal animals like the elephant. It has been urged, however, that the Chellean boucher was too thick to be comfortably hafted, and that when secured in its place, as we must assume by cord or animal sinews, it would be almost concealed in its wrappings, and present a very clumsy appearance. It would have been a deadly weapon all the same.

The finest examples of the boucher are made of flint ; with less tractable material, such as quartzite, the result is extremely crude.

The boucher is frequently found unaccompanied by other implements ; this is the case at the famous locality of Chellés on the banks of the Seine (Seine-et-Marne), where abundant examples of the boucher occur without any admixture of other forms.¹ This has led to the view that the boucher was the one and only implement of Chellean man. We now know, however, of several localities, notably at St. Acheul and in Belgium, where other kinds of implements occur along with it : the researches of M. Rutot in particular have made us familiar with Chellean scrapers, chisels, and a dagger, recalling that of Strepy, but dressed all over, haft as well as blade, and of superior workmanship.

Our knowledge of the Tasmanians would lead us to expect that Chellean man was in any case armed with the spear ; and that this was the case seems to be proved not only by the occasional occurrence of hollow scrapers, but, still more to the point, of flint lance-heads, from which it would appear that already at this early period a distinct advance had been made on the Tasmanian weapon by providing it with a more efficient point. This may well have been produced in response to the more rigorous demands of the environment. A still greater advance is indicated by the discovery of worked flakes which M. Rutot interprets as arrow heads.²

The Chellean implements occur most commonly in river

¹ It is doubtful whether this statement can be fully sustained.

² M. Obermaier discredits both these and the lance-heads ; he states, however, that hollow scrapers occur also at St. Acheul : *Mitth. d. prahistorischen Kom. d. K. Ak. Wiss. Wien.* Bd. ii. 1908.

gravels, though some examples are known from caves, as in the famous Kent's hole, near Torquay, which has yielded rough bouchers from the lowest layer along with teeth of the cave-bear.

As regards the geographical distribution of the Chellean industry, the characteristic boucher is found in all the continents of the world except Australia. It is spread over the whole of France, all that part of England which lay south of the region of most persistent glaciation, and it is found in the river gravels of Belgium.

It was in England, as we may note in passing, that the boucher first attracted attention. Bagford and Hearne figured a boucher of Acheulean type in 1715.¹ Frere wrote a memoir on those he had discovered at Hoxne in Suffolk, which is remarkable for its insight and exactitude (C. Lyell, *The Antiquity of Man*. London, 1863, p. 168).

The Chellean boucher is not found in that part of European Russia which was covered by the ice, but outside this limit it is met with in the Crimea. It is rare in the north of Italy, but becomes abundant as we go south. A few examples have been found in Portugal, and great numbers in Spain, as at San Isidro, near Madrid.

It has been traced from end to end of Africa, in Egypt, the Congo, Rhodesia, the Transvaal, and the Cape; and from west to east of southern Asia, Arabia, Palestine, the valleys of the Tigris and Euphrates, to the Narbadda valley in Hindustan, where it is represented by quartzite implements which occur in company with extinct species of elephant (*E. namadicus* and *E. stegodon insignis*) and two species of hippopotamus, and still more to the east in Cochin China and Malacca.

In North America it is scattered over the greater part of Canada and the United States, where it is associated with two species of extinct elephants (*E. Columbi* and *E. Jacksoni*) allied to *E. antiquus*, but apparently of later date. It is said to occur in South America also.

Thus, if we except Australia, Tasmania, and Oceania, the distribution of the Chellean industry is world-wide.

It is necessary to proceed with great caution in drawing inferences from this fact.

¹ L. Capitan, "La première hache Acheuleanne connue," *Rev. Éc. d'Anthr. Paris*, 1901, p. 219.

In the first place, the Chellean industry cannot be identified with Chellean man; that is, we have no reason to assume that even at this early date the human family was homogeneous, not yet differentiated into divergent races. General considerations would indeed suggest the contrary.

In the next place, we cannot identify the Chellean industry with a Chellean epoch. It probably originated at some particular centre and then travelled in a slowly enlarging wave over the entire world; it is even possible that fresh industries had already arisen while this wave was in progress, and that these were similarly propagated, so that after a sufficient interval of time all the various palæolithic industries might have existed simultaneously in different parts of the earth.

It may be pointed out in illustration that at the time we were living in an age of iron we found the Fijians and many other races still using implements of polished stone, and the Tasmanians, palæolithic, or still ruder implements.

The duration of each of the several epochs may be defined on the one hand by its first appearance, and on the other by the first appearance of that next succeeding it. Thus with the advent of the Acheulean, the Chellean epoch may be regarded as closed; nevertheless the Chellean industry may persist after this in some other locality, a fact which may be expressed in the statement that the Chellean industry survived into Acheulean or even later times. Thus the industries overlap the epochs.

The Chellean fauna.—This will naturally have differed in different parts of the world, and even in Europe geographical provinces may have been defined. According to French observers, the fauna which accompanies the bouchers at Chelles is distinguished by the presence of *Elephas antiquus* and the absence of the mammoth (*E. primigenius*). As an almost inseparable companion of *E. antiquus* we find also the small-nosed rhinoceros (*R. Merckii* or *leptorhinus*), and among other distinctive animals we may mention the hippopotamus. These are all southern forms indicative of a warm climate.

In Belgium and England the case appears to be different, since, in addition to the animals just mentioned, the fauna of the succeeding Acheulean stage, in particular the mammoth and the woolly rhinoceros (*R. tichorhinus*), also occur, their bones lying intermingled.

In Italy, on the other hand, the fauna of the mammoth is

unknown, at all events south of Piedmont, but the fauna of *E. antiquus*, on the other hand, is both richly represented and very persistent, surviving into the Mousterian stage.

In the northern countries where both faunas occur it has been suggested that their intermixture may be due to remanement, *i.e.* to the destruction of an earlier deposit and its redistribution, along with its contained fossils, in a later deposit. Prof. Boyd Dawkins has attributed it to seasonal migrations.

The Acheulean Stage

The Acheulean industry is the direct descendant of the Chellean, and the boucher is still the characteristic implement, but it is a different boucher, distinguished by its finer workmanship and more elegant form. It is much flatter, not so thick, and consequently lighter; the flaking is not so coarse, and the edge has been worked by repeated minute retouches into an even, regular line, very different from the jagged edge of the Chellean form. It was thus rendered more trenchant, so that the Acheulean boucher is not only a better finished but a more efficient implement.

The same kind of refinement is seen in the scrapers and chisels, and it leads on to the Mousterian point, to be described presently, which makes its first appearance in the Acheulean stage anticipatory to its abundant development later on.

The Acheulean fauna, even in France, is a mixed fauna; it includes both the southern forms characterised by *Elephas antiquus*, and the northern, equally characterised by the mammoth.

The mammoth, though not so large as its predecessor *E. antiquus*, was larger than either of the existing species of elephant, and attained a height of over 16 ft. Its tusks, large and strongly recurved, were sometimes as much as 23 ft. in length. Its warm coat of close fur and long hair no doubt served in the first place as a protection against the rigours of a cold climate: a further protection was afforded by a layer of fat 8 cm. in thickness beneath the skin. Its teeth, by which it is generally recognised in the fossil state, differ from those of *E. antiquus* by their greater breadth and more numerous transverse lamellæ; the lamellæ themselves are narrower and crowded closer together; the enamel on their surface is

less strongly folded. This species was more nearly allied to the Indian than the African elephant.

Its companion, the woolly rhinoceros (*R. tichorhinus*), was also well protected from the weather by a coat of fine wool and long hair. The woolly rhinoceros was armed with two large horns, which stood one behind the other over the nose: the larger sometimes reached a length of three feet. To carry the weight of this the septum between the nostrils was converted into bone, and this more complete ossification of the nasal septum is one of the features which distinguishes the woolly rhinoceros from *Rhinoceros leptorhinus*.

The incoming of this new fauna points unmistakably to a change in climate. A fresh strain was put upon the faculties of primitive man, who had now not only wild beasts to contend with, but an increasing severity of the seasons, especially in winter. Perhaps it was for this reason that he began to make his home in caverns.

The Mousterian Stage

The Mousterian industry is not quite so sharply defined as could be wished. It is characterised in the first place by the absence of the Acheulean boucher, although, unfortunately, this absence is not absolute; and next, by the abundant presence of the Mousterian point. In making this the operator began by dressing the face of the intended point while it still formed part of the parent nodule or nucleus. This done, it was struck off by a single blow with a stone hammer along a face parallel to that already dressed. As a consequence this second face, which is destitute of secondary chipping, is always marked by a bulb of percussion; that is, by the central swelling of a concentrically wavy surface (conchoidal surface) marking the spot where the final blow detached the flake from the block. The Mousterian point varies considerably in size—two and a half inches is about the average length, but the maximum is about six inches. The edges are often carefully retouched. The larger examples might be regarded as a refined form of the boucher.

The scraper of the Mousterian stage possesses certain special features, generally presenting a finely worked curvilinear edge.

The industry takes its name from the cavern of Le Moustier,

in the valley of the Vézère, Dordogne, and it is in caverns that it is mostly found. In France and Belgium it usually occurs in the lowest layer of the implement-bearing *débris*.

In Italy it occurs also in gravels in company with *Elephas antiquus* and *Rhinoceros Merckii*, hippopotamus, bears, and lions. In the grotto de San Ciro, near Palermo, the bones of hippopotami were found in such quantity that according to a careful estimate they must have represented the remains of at least 2,000 individuals.

We have now obtained a glimpse of the environment within which man courageously carried on his long struggle for existence amidst a crowd of competing beasts of prey. We have examined his weapons, and followed their improvement as they progress from stage to stage; and though we willingly admit their testimony to his intelligence and skill, we cannot help feeling that they are but slight provision after all for combat with the lion, the rhinoceros, or the cave-bear. In comparison with these animals they leave him still far inferior in brute force. All the more reason have we to assume that he owed his successful survival in the first place to his intellectual and moral gifts; his powers of observation, his ingenuity in device, his faculty of combining strength by association—these were the weapons, quite as much as his stone points and spears, which obtained him dominion over all the beasts of the field.

This being arouses our curiosity, and we are, above all, anxious to know in what relation he stood to ourselves. Fortunately, the last half-century has put us in possession of some few of his bones, no more than would fill, say, a single coffin, but they are sufficient to instruct us on most of the points of capital importance.

The first discovery of the bones of lower palæolithic man to receive serious attention was made in 1856. Not far from Düsseldorf, in Rhenish Prussia, the valley of the Dussel forms a steep and narrow ravine known as the Neandertal. Its rocky walls of limestone are penetrated by several caves, which owe their origin to the solvent power of running water. In one of these caves, opening some 60 ft. above the present level of the river, which has sunk its bed to this depth since the cave was inhabited by man, the famous Neandertal skeleton was found. It lay embedded in a hard, consolidated

loam, and when first exposed by the workmen who were quarrying the limestone was probably complete. Unfortunately, it suffered great injury from their hands, for they had, of course, no conception of its value; but by the opportune intervention of Dr. Fuhlrott, the thigh bones, the upper bone of each arm, shoulder-blade, collar-bone, some fragments of ribs and, most precious of all, the skull-cap or brain-pan, were rescued from destruction, and are now exhibited in the University museum at Bonn.

More than fifty years have elapsed since this momentous discovery was made, and from that time to this it has continued to occupy a foremost place in the investigations of anatomists. When its discoverer first exhibited his specimens before a meeting of German anthropologists at Bonn, doubts were freely expressed as to their human character, and subsequently the famous anatomist Virchow endeavoured to explain away the remarkable features of the skull-cap by attributing them to disease. Huxley, whose fame, notwithstanding his brilliance as a writer, will always rest on his genius as an anatomist, arrived at conclusions which we now perceive to have made the closest approach to the truth. He recognised the skull as truly human, but at the same time as the most ape-like he had ever beheld, and placed it below the Australian, which he regarded as its nearest existing representative.

So long, however, as this skull was the only one of its kind, its testimony failed to produce complete conviction: its age was open to question, for the fauna of *Rhinoceros Merckii*, though occurring in a similar cave only 130 paces distant, had not been found in actual association with the skeleton itself. It might have belonged to an abnormal individual, great as were the chances against such an accident, and, finally, its completeness left something to be desired. Very welcome, therefore, were the fresh discoveries which followed from time to time down to 1905; these, while largely adding to our knowledge, unite to confirm the judgment of Huxley expressed in 1863.

The material now accessible to study includes the following: A lower jaw from La Naulette, found in 1866; part of a lower jaw from Sipka, 1879; two nearly complete skeletons from Spy, 1885; a lower jaw from Malarnaud, 1889;

and various fragments representing perhaps a dozen individuals from Krapina, in Croatia. All these remains, though distributed over a wide geographical area, are characterised by similar peculiarities; and by combining the evidence they afford we are able to reconstruct the skeleton of lower palæolithic or Neandertal man. Wherever the evidence overlaps, it is found to correspond, thus confirming our conclusions and dissipating the mistrust which very naturally prevailed when the Neandertal skeleton was the only one known.

The face, to which we involuntarily turn to gain our first impression of the man, presents a singular aspect, unlike that of any existing race. One of the most salient features is the prominent ridge which extends continuously from temple to temple at the base of the forehead; it is formed by an excessive growth of the brow ridges, the supra-temporal ridges, and the glabella, the latter a prominence of the forehead immediately above the root of the nose. These several regions are not only greatly developed, but they have become completely confluent, forming a single ridge, which we may speak of as the frontal torus. The only existing race in which the frontal torus at all approaches that of the Neandertal skull is the Australian, and even this does so only remotely.¹ In the Australian skull the torus is rarely, if ever, so completely continuous and uniform as in the Neandertal; its dimensions are less and its characters different. In the Neandertal skull the torus receives additional emphasis from the presence of a corresponding depression which runs parallel with it along its upper margin. This trough is spoken of as the frontal fossa; nothing resembling it occurs in the Australian skull. In the Australian skull it is the glabellar region of the torus that is most protuberant, projecting farthest immediately above the root of the nose, which looks as if it had been squeezed in close under the glabella: this gives an appearance of concentration to the Australian face. In the Neandertal skull the torus does not descend in this fashion: it rises well above the eyes and root of the nose, recalling its disposition in the chimpanzee.

¹ See, however, D. J. Cunningham, "The Evolution of the Eyebrow Region of the Forehead; with Special Reference to the excessive supra-orbital Development in the Neandertal Race," *Trans. Roy. Soc. Edin.* 1908, xlvii., pp. 283-311, 3 pls.

The orbits are large and round, and rise upwards, encroaching on the forehead.

The nasal aperture is remarkable for its great size, particularly in breadth; the nasal bones are broad and concave upwards, and the sides of the nose pass backwards into the cheeks without the marked distinction which occurs in recent races. Thus, although the soft parts of the nose have disappeared, we may conjecture that this organ was of unusual dimensions; it probably projected in a snout-like fashion of its own, not comparable with anything we know either among men or the man-like apes.

The distance from the root of the nose to the mouth was greater than in any existing human race.

In the imaginary restorations which have from time to time been ventured on by painters and sculptors, the face is always represented as prognathous, that is, with projecting jaws. This is simply a bad guess, prompted by a mistaken analogy. We now know, from observations made on the Krapina fragments and the Gibraltar skull, that the face was truly orthognathous, as much so as in many a civilised white man. At the same time, the jaws present some remarkable peculiarities: they are large and parallel-sided; the lower jaw in particular is heavy and massive and especially distinguished by the absence of a chin. In the existing lower races of mankind the chin is often notably reduced in size, but never completely suppressed. The Neandertal race in this respect mark a passage from the higher apes. Further than this, the little bony processes which lie within the angle of the jaw and give attachment to the muscles of the tongue concerned in speech appear to be missing from the Neandertal jaw; and from this it has been inferred that the power of speech was not fully developed.

The teeth present as many Simian characters as the jaws; the incisors are small, the canines very large, and the premolars are very oblique. In ourselves the three grinding teeth, or molars, diminish in size and importance from before backwards, the third or wisdom tooth being the smallest, and sometimes even undeveloped. In the Neandertal race the order of magnitude is reversed, and the wisdom tooth is the largest of the three.

The brain-pan is consistent with the face: the swollen frontal torus and its accompanying fossa have already been alluded

to. Beyond the fossa the forehead is receding, and the skull rises to a comparatively low vertex; the occiput is distinguished by a similar slope in the opposite direction, and swells into a strong ridge for the attachment of the neck muscles. The walls of the skull are thick, and the thickness of the frontal region is prodigious. In this region the floor of the skull rises up to an unusual extent, so that, owing to this and the thickness of the frontal bones, the space left for the frontal lobes of the brain is very much diminished. It is in these lobes that the faculty of speech is lodged.

Notwithstanding these indications of inferiority, the capacity of the skull is considerable; it cannot be much less on an average than 1,250 cc. This is about the same as the average for the Australian skull, 1,230 cc., so that, apart from the quality of the brains, of which we know nothing, the Australian and the Neandertal races are on the same level.

Not only in this respect, though it is the most important, but in a great number of other characters the Australians make the nearest approach to Neandertal man. Many of the more brutal Australians, especially among those inhabiting the south of the continent, present a depressed cranial vault with receding forehead and occiput, almost identical in profile with some forms of Neandertal skull: there is a resemblance, though not identity, in the characters of the frontal torus; and the lower jaws, with the teeth, present some analogies. The Australians are not the same race as the Neandertal, but at the same time they are more closely allied to it than any other; and we may therefore regard the Australian as in a special sense the modified descendant of early palæolithic man.

We have spoken of the Neandertal race in general terms as lower palæolithic, and it is difficult to be more precise in the present state of the evidence. As we have seen, the Neandertal skull itself is undated, and nothing is known of the age of the Gibraltar skull—the only example in which the face is preserved in its natural relation to the rest of the skull; but of the remaining specimens we are better informed.

Spy.—The cavern in which the two skulls of *Spy* were discovered by Professor Max Lohest is situated in Carboniferous limestone forming a wooded hill above the Orneau, in the commune of *Spy*. Near its mouth lay a pile of débris composed of several layers. In the lowest layer, containing very rough

Mousterian points and the fauna of the mammoth (*E. primigenius*, *R. tichorhinus*, *Ursus spelæus*, *Hyæna spelæa*, etc.), two fragmentary human skeletons were found, the remains of two individuals who had evidently been killed by a fall of stones from the roof.

Le Trou de La Naulette.—On both sides of the valley of the Lesse, just above its confluence with the Meuse, several caverns open near the middle of its slope, at heights of from 75 to 100 feet above the river. The cave of La Naulette is one of these; it was flooded during the lower palæolithic epoch at irregular intervals by the Lesse, when that river flowed at a height of about 90 feet above its present level. Each inundation left a deposit of loam on the floor of the cave, and the time which elapsed between successive inundations was sufficient to allow of the growth of an incrustation of stalagmite; there are seven of these stalagmite floors and seven layers of loam. At a depth of 15 feet below the lowest stalagmite the famous jaw of La Naulette was found. Its Simian characters led some anatomists, amongst them the famous Virchow, to deny that it was human; but the subsequent discoveries at Spy and Krapina leave no doubt on this point, and we now recognise it as appropriate to the Neandertal skull.

The bones of the other animals found in this cave mark the fauna of the mammoth.

Krapina.—The hollow in which the ossiferous deposits occur at this locality is not so much a cave as a recess, which was excavated by the river Krapinica, as it washed against a cliff of friable Miocene sandstone. Since accomplishing this work the river has sunk its bed 82 feet below the floor of the recess; and the recess itself is now completely filled with débris. At the base is a layer of pebbles left by the river; over this lie sand and loam, partly deposited by flood waters, partly formed by dust weathered from the walls: fallen angular fragments are scattered throughout. Here and there lenticular layers, dark grey and red in colour, are intercalated with this material. They mark the site of successive occupations by man; burnt sandstone, charcoal, broken and burnt bones and stone implements are found in them. The lowest layer seems to indicate a dwelling-place; it contains the fragmentary remains of ten or twelve individuals of different ages, children and adults. The bones are all broken and more or less burnt; and on this evidence some have suspected

Neandertal man of cannibalism. It is quite possible for hunting tribes to eat their kind under stress of hunger without, however, being addicted to the practice, so that if in this case we have in fact before us the remains of a cannibal feast we cannot infer from it that cannibalism was the universal practice of the age.

The fauna of this station includes *Rhinoceros Merckii*, *Ursus spelæus*, and *Bos primigenius*. The implements are rough flakes of Mousterian type; some have been made out of the pebbles of the Krapinica river, and retain a part of their original surface. Some rude implements of bone are said to occur with them, one of which has been spoken of as a bone "axe."

The discoverer of these relics, Professor Georganovic-Kramberger, regards them as older than the last mountain movements which have affected the district.

It will be perceived from the summary just given that the evidence is extremely conflicting: the fauna speaks with two voices. *Elephas antiquus* points to a Chellean age; the mammoth to the Acheulean or Mousterian. The industry is open to question. We call it Mousterian; but in some cases, as at Krapina, it is so rude that we should not be surprised if it proved to be older; though in that case the absence of bouchers would have to be accounted for.

The most puzzling fact is the association of the same race of men with two distinct faunas, which are supposed not to be contemporaneous. We have seen, however, that *Elephas antiquus* continued to exist in Italy, while the mammoth prevailed in France and Belgium, and it might be suggested that *Rhinoceros Merckii* lingered on longer in Croatia than in regions more to the north and west on the other side of the Alps. There are convincing arguments, however, against this view. Professor Penck proposes to meet the difficulty by supposing that the *Elephas antiquus* fauna, after yielding to the mammoth, once more returned, and extended over Europe during a warm interglacial episode.

If this were the case, then the suggestion of a Chellean age, which is afforded by the occasional presence of the *Elephas antiquus* fauna with Neandertal remains, loses its force; and if we are to depend on the implements alone we shall be compelled to concede, on admittedly imperfect data, an antiquity no more remote than the Mousterian stage.

Thus it would seem that the earliest race in Europe of whose

bodily remains we have any knowledge is the Mousterian; of Chellean or Acheulean man—presumably more primitive—nothing is left but his handiwork. However remotely the Australians may be related to the Neandertal race, they at all events possess one character of the highest importance in common—their average cranial capacity is almost identical. In this respect the lower races of Australia stand at the present day on the same level as the men of the Mousterian stage. In stature they do not differ widely, the average height of the Australian being 1,668 mm. (5'47 feet), of the Neandertal race, 1,625 mm. (5'3 feet). We have already mentioned some of the characters of the skull and face of the two races; as regards the hair we can speak only of the Australians. Their hair is wavy, and they are therefore included in the same subdivision of mankind as ourselves, *i.e.* the Cymenrichi. They further resemble us in the abundant growth of hair over the lower part of the face.

In the arts they show a decided advance beyond the Tasmanians. The spear, though it still continues to be the most important weapon, is more complicated; it is frequently provided with barbs, and the head is not of one piece with the shaft, but a separate part made of hard wood or flaked stone. They are without the bow, but, on the other hand, they possess a throwing stick for hurling the spear, and two kinds of boomerangs, one of which returns in its flight. Their stone adzes and axes are provided with a haft, and their stone knives with a wooden handle.

The art of manufacturing the stone implements has been carefully observed and described. The axe is made in more ways than one; sometimes a fragment of a jointed rock or a pebble from the brook is selected as making a sufficient approach to the desired size and shape, and then dressed, with a small pebble used as a hammer, to a sharp edge at one end. In other cases the work is begun by striking off a large flake from a block of stone; holding this in the left hand, with the conchoidal surface turned away from him, the operator then dresses it by blows delivered on the side facing him.

The knife is also obtained by flaking; a block of stone about eight inches long by six broad, fairly flat at one end and tapering to the other, is held upright against the ground and struck nearly vertically with smart blows near the edge. In this way, long, thin lamina, something like the blade of a dagger, are

obtained, triangular in section, with a single broad face on one side and two narrower ones on the other. As might be expected, they vary considerably in form: some are broad and lanceolate, others narrow and elongate, but all are used for the same purposes.

These flakes resemble in every respect those which we shall encounter later on in the Magdalenian stage; but the Australians make other flaked implements, which strongly recall the still later Solutrian.

Spencer and Gillen, in describing the manufacture of the Australian stone implements, remark that "some men are much more skilful than others." To this it may be added that measurement of the skull shows a wide range in cerebral capacity: there is a difference of at least 400 cc. between the highest and the lowest examples. But when we turn to palæolithic man similar differences as regards workmanship reveal themselves. No one can look through a collection of implements from the same locality, even when these are Chellean or Acheulean bouchers, without being struck with their extraordinary difference in style and finish: in some cases we seem to have before us the work of a novice or mere bungler, in others our admiration is aroused by truth in form and accuracy in detail, where every stroke speaks of the master hand. Thus the earliest records of our kind, as much as the facts of daily experience, offer a contradiction to that amazing doctrine which asserts the equality of individual men. Not equality but inequality is the universal and constant law; and it is this variation in endowment which lies at the base of all progress, whether in the human or the animal world.

The same observers also inform us that there are certain localities where the best knives are made, and that for every flake considered good enough to use at least a score are discarded. This also finds a parallel in palæolithic times; for in several localities, both in England and abroad, factories of bouchers and other implements have been unearthed, where every stage, from the rough nodule to the finished product, have been observed, as well as abundant wastrels.

Some of the Australian axes, made of a close-grained diorite, are ground down, after they have been chipped into shape, on a flat slab of sandstone, with the aid of sand and water. Polished implements such as these are supposed to be the

exclusive characteristic of the neolithic period; but as the Australians are still in a palæolithic stage of culture, they present us in this case with an exception, for which various explanations may be found.

Bone is used for some implements, such as awls and gouges; the fibula of the kangaroo or emu when ground down to a fine point makes an excellent awl, which is used for piercing holes in skins, preparatory to "sewing" them together with the sinews of animals. We shall meet with bone implements in deposits of the upper palæolithic period.

The Australians, unlike the Tasmanians, are acquainted with the art of fishing, using for this purpose special spears provided with several points, or, in some parts of the continent, actual fish-hooks, which are made of wood or shell.

A wooden rod about the size and shape of a lead pencil serves as a comb. A heavy wooden stick, chisel-like at one end and pointed at the other, is used by the women for digging up yams and other roots. On occasion, as in household brawls, it comes in handy as a weapon.

The Australians are quite at home in the water; they are expert swimmers and divers, and they know how to make and handle several kinds of water-going craft. The rudest of these is a raft, made up of bundles of rushes, such as the Tasmanians possessed. Another raft is constructed of the trunks of trees, two or three, 15 to 20 feet in length, being lashed together: on this two or three persons may be paddled or punted across a river. Rafts were probably used at a very early stage of human culture; but the birch boat, which the Australians also possess, takes us at once to a higher level of development. This is generally made by carefully removing with a stone axe the bark of a single tree, generally a eucalyptus, known as the red gum; struts are placed inside to open it out, and it is propped up by sticks placed at the bow and stern; the ends are ingeniously tied up with string furnished from the bark of another eucalyptus (stringy bark), and after being left to dry for about a fortnight the boat is ready to be launched. Sometimes a lump of clay is placed at the bottom of the canoe, and on this a fire is lighted, which gives warmth to the fisherman and serves to cook his catch.

Their huts, though very rude, show some advance on the Tasmanian wind-screen; but they are seldom occupied for more

than two days at a time, unless fish is plentiful, or certain vegetables are in season. In some cases caves or rock-shelters are used as temporary dwelling-places.

Though accustomed to wander in a state of nakedness, except for a hip girdle, in which the men carry their weapons, and to which the unmarried girls attach a narrow little apron or fringe made of strips of fur, yet in camp or on cold nights they put on warm clothes. These are made from the skins of the kangaroo, wallaby, opossum, native "bear" and native "cat"; 30; or 40 opossum skins are required to make a cloak for an adult. After the skin is removed from the animal it is pegged out fairly tight on a hard patch of ground, and, when dry, trimmed into a rectangular shape with a stone knife; the inside is then carefully gone over with a stone scraper, to remove all traces of fat and flesh; and finally a mixture of grease and red ochre is well rubbed in. Holes are pricked with a bone awl, and through these fine sinews taken from the animal which has supplied the fell can be threaded; in this primitive fashion the skins are "sewn" together.

Besides the cloak, which serves for use, they wear many adornments: necklaces of various kinds, among which may be specially mentioned those made of univalve shells or kangaroo teeth. The shells are perforated by a stone point and threaded together by passing a string through the mouth of the shell and the perforation; as a consequence, they do not hang in regular arrangement, but point in all directions.

Though the Australians have not developed a system of writing, yet they make use of signs marked on their "message sticks." These sticks are carried by messengers to identify them in their office, and the signs upon them serve as a rude kind of *memoria technica* to recall the message.

A complicated system of religious rites governs the Australians in all their conduct in life. A belief in magic is deeply rooted; in this connection the account given by Spencer and Baldwin of their magic stones or "churinga" is especially interesting.

It would thus appear from the foregoing account that the Australian natives, though still remaining in the palæolithic stage, have made a considerable advance on the culture of Neandertal man. This they may easily have accomplished by their own efforts, though it is possible, at the same time, that they

may have borrowed something from adjacent races; occasional visitors from the outlying islands might easily reach the mainland by canoes. In the lower palæolithic epoch their ancestral representatives were spread far and wide over Europe, and the corresponding stage of culture was distributed more widely still: now they are confined to an isolated continent in the far south. Hence we may suppose either that the Neandertal race was driven by stress of circumstances out of Europe, and wandered till it reached the Australian region; or that at some early time it occupied a tract of land continuously extending from Europe to Australia, and has since been everywhere blotted out except in its southern home. We cannot appeal to the widespread distribution of Chellean or other implements in favour of either theory, for, as cannot too frequently be repeated, the possession of a common culture is no proof of community of race. To suppose that is so is to repeat the error of the philologists, who endeavoured to identify races by language. On the other hand, the sporadic occurrence of individuals with Australoid characters in the Pacific, and the existence of related races such as the Veddahs and the Ainos in areas so widely separated as India and Japan, is highly suggestive, and would seem to indicate the extension of a primitive race allied to the Australian over a great part of the old world.

If, as we have supposed, the Tasmanians were driven out of Australia by a lower palæolithic race, now represented by the Australians, it is evident that the two most divergent subdivisions of the human family, that is, the *Cymentrichi* and the *Ulotrichi*, were already in existence at a very early date; and we shall soon encounter important evidence pointing to the existence of the *Ulotrichi* at a later period, that is, during upper palæolithic times, in Europe itself.

REVIEWS

Electrical Engineering. By W. SLINGO and A. BROOKER. New edition, thoroughly revised by W. Slingo and T. F. Wall, M.Sc. [Pp. 850 + viii., 389 figures.] (Longmans, Green & Co. 12s. 6d.)

THE success of this well-known work has necessitated some ten editions and reprints since its first appearance in 1890, and it must be said at once that prosperity has not dulled the authors' efforts to keep their book abreast of the times. Electrical knowledge has progressed, more especially on its applied side, at such an amazing rate, that a heavy burden is laid on the writers of descriptive text-books who desire to do their duty by their readers. Progress entails extensive additions to almost every section of a new edition, and rewriting, not mere revising, has to be the treatment prescribed for much that had been offered previously.

The electrician of to-day can scarcely complain of the way in which his wants are ministered to. Treatises general and special, text-books, and "short-roads" abound. Weekly journals have sprung up with mushroom-like rapidity and insist on adding to his knowledge.

The volume before us aims at giving a sound treatment of direct and alternating currents, with their immediate applications. These in themselves are sufficiently comprehensive to require upwards of 850 pages of closely printed matter. Some 300 pages are devoted to introductory theory, together with accounts of laboratory and workshop instruments; thus, for example, the Bridge-Megger, the Crompton Potentiometer, the Evershed Ammeter, the various Weston instruments, and the Kelvin Volt Balance are dealt with among much besides.

Succeeding chapters discourse on alternating and direct current dynamos, motors (with digressions on electric traction on the London County Council's tramways and the South and Central London Railways), transformers, storage cells, and arc incandescent lamps.

General criticism can serve little purpose in dealing with a work which can fairly claim to have seen active service; but the descriptive portions of the book strike us as rising in excellence above the general level of the rest. Perhaps in the midst of so much that is good, we may be allowed to make a few points, not so much as criticisms, but rather as suggestions.

The otherwise admirable section on photometry cannot claim to be complete without a notice of flicker photometers. More stress might well be laid on the advantages of the Weston normal cell over the Clark. The electromotive force of the latter, given throughout as 1.434 volts, needs amending to 1.433 volts—its now accepted value. The H pattern of cell should replace the one mentioned as the standard form. Stress is not laid on the fact that Ohm's law merely embodies the results of experiments with metallic conductors, and that through its existence we need to define as primary only two of the three units of current, resistance and electromotive force, the two taken being those of current and resistance. Manganin should most certainly be added to the description of alloys suitable for resistance coils.

A short account of rectifiers or "electric valves" would be of value in the section on alternating currents, which, by the way, cannot afford to ignore the oscillograph and cathode-ray detectors which give experimental illustration of the character and form of alternating currents under different conditions. Such devices have great utility and bring the subject home to the student in vivid fashion.

A small correction may as well be inserted in the account of the Central London Railways electrical equipment. The company has not for some years hauled its trains by independent locomotives, as is here remarked.

We take exception to the form of the statement (p. 652) that "pure white light is in reality composed of rays of seven different colours—red, orange, yellow, green," etc. The intention is of course evident and well meant, but the statement (repeated elsewhere) may find a student who would leave it with quite a wrong conception of the infinite gradation of the colours in the luminous spectrum. With this very slight exception the section on lighting is admirable.

The incandescent gas mantle invented by Auer von Welsbach suddenly reduced the cost of gas lighting to one-third of what it had been before. Electric lighting received a temporary set-back, but has now come into its own (again primarily at Welsbach's hands) with the invention of the new metallic filament lamps, the best of which boast about three times the efficiency of carbon lamps and a longer life. Such lamps on low voltage circuits offer very real competition to gas. Tantalum lamps we find adequately treated in the work before us, but it can fairly be said that the recent remarkable development in wire lamps has depended chiefly on the introduction of the different types of tungsten lamps. Such lamps have an efficiency per candle power not far from double that of tantalum lamps; their superiority is largely due to the higher radiating properties and to the very high positive temperature coefficient of resistance of tungsten. The next revision which this book undergoes will entail drastic rewriting of the section on incandescent lamps.

We were glad to find a full account of Mansbridge's tin-foiled paper method of constructing condensers. Nothing, however, is said as to the construction and reliability of mica condensers, nor as to the attempts which have only recently resulted in producing for the first time really serviceable and efficient glass condensers, such as those of the Moscicki type, which represent probably the most satisfactory and practical high-tension condenser for commercial work now on the market.

Enough has been said to illustrate the difficulties which beset the authors of a book which covers much ground in a very progressive applied science. There is no trouble in suggesting alterations in many sections since this book went to press. But the fact must not, and cannot, be held in disparagement of the material offered to us by the authors, for as to its excellence there can be no doubt. We noticed but few misprints, none of them important. The style throughout is attractive, and the authors write with a facile pen and a proper appreciation of a student's dilemmas.

G. W. C. KAYE.

An Introduction to Electricity. By BRUNO KOLBE. [Pp. 430.] (Kegan Paul, Trench, Trübner & Co. 10s. 6d.)

An Introduction to Electricity is a translation of the second German edition of Prof. Kolbe's lectures delivered at St. Petersburg. It is divided into two sections, dealing respectively with static and dynamic electricity, and concludes with an appendix containing Historical Remarks, Repairs, and Practical Hints. The sequence followed is good, and the experiments are usually well described,

but the theory and explanations are sometimes not quite clear enough. In the first part of the book no explanation is given as to when an electroscope shows density or when it shows potential (called degree of electrification), and the capacity of the electroscope is not mentioned. The second part of the book, on dynamic electricity, covers a large field, and the treatment is rather inadequate. Very little space is devoted to magnetism, and this subject is only treated qualitatively. Throughout the book, which is written in the first person, Prof. Kolbe seems rather unorthodox in his choice of terms. For instance, "for the sake of clearness" he substitutes "fall of current" for "fall of potential," and uses "current intensity" for "current." Several of the terms used must be unfamiliar to English students: for instance, positive and negative electricity are called electricity of the first kind and electricity of the second kind, and the middle of a mass of lodestone where no iron filings cling is called the zone of indifference. The treatment throughout is unmathematical—in fact, the book is intended for those whose mathematical equipment is small. The practice, however, of dealing with subjects such as Hertzian waves and Radium rays in a text-book before defining, say, the moment of a magnet, is unusual. To an inexperienced lecturer without access to other books the details of many of the experiments should be of use, but the book is not one which can be recommended to a student entering upon a study of electricity and magnetism.

R. W. FORSYTH.

Experimental Elasticity. By G. F. C. SEARLE, M.A., F.R.S. [Pp. xvi. + 183.] (Cambridge University Press. 5s. net.)

THE volume is the first of a series of text-books on Practical Physics which the author proposes to publish. The usefulness of books of this type is obvious, and this one will doubtless supply the demand in the particular subject with which it deals.

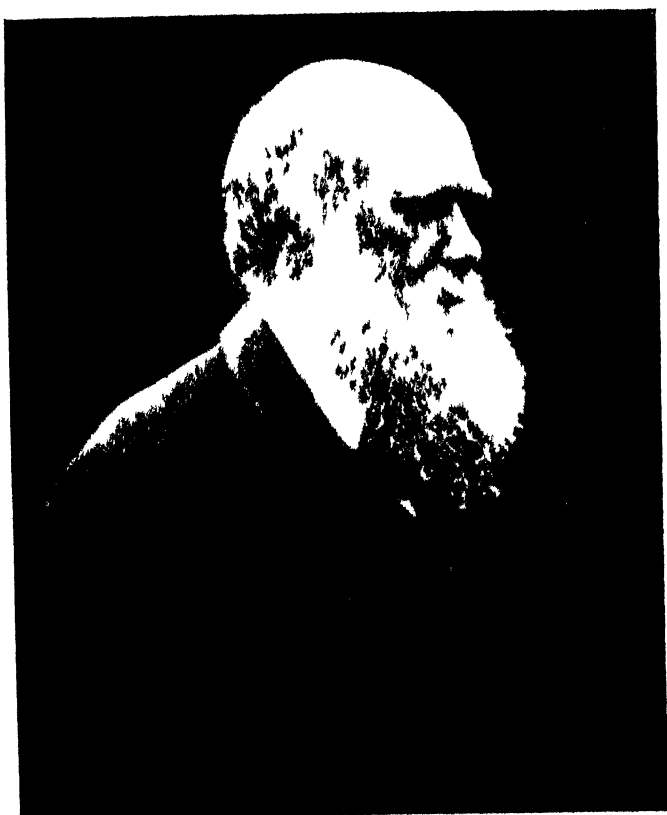
It is divided into three sections. The first deals with the definitions of the elastic constants and the elements of the theory of elasticity. Some problems having a bearing on the experimental work are solved in the second chapter; and the third contains descriptions and directions for the performance of some fourteen experiments on elasticity.

The student thus finds in one text-book both the method of carrying out the experiment and the theory involved in it. This is certainly a distinct advantage. In each case the apparatus is carefully described, and copious details as to experimental difficulties and means of surmounting them are given. Besides this, examples of the best method of recording observations and results are shown, and the description of each experiment concludes with a practical example based on actual measurements made either by Mr. Searle himself or his colleagues.

The book concludes with a series of notes, the last of which—"Hints on Practical Work in Physics"—deserves the careful attention of students. The author rightly insists that good work is only possible when observations are carefully made and all neatly recorded at once, all calculations, however simple, being carried out after the completion of the experiment.

This work should prove very useful not only to the students of physics themselves, but also to those whose duty it is to prepare experiments for them and instruct them in the use of apparatus.

A. O. RANKINE.



CHARLES DARWIN

1809—1882

[Irontispiece

DARWIN AND HIS PUBLISHER

By JOHN MURRAY, J.P., D.L., F.S.A.

THE relations between authors and publishers have long formed the subject of satire and ridicule in prose and verse. During the past twenty years, however, the appearance of the memoirs of several of the leading publishers has shown that they, as a class, and as individuals, are not more deeply tainted by original sin than are the authors; and that cordial relations have been the rule, disputes the rare exception.

I have been asked to write a short account of the dealings of my father with Charles Darwin, and I assent with the less hesitation because it seems to me that those dealings exhibit a type of what such an association should be—creditable to both parties as gentlemen and men of business. Everything is discussed openly and frankly between them; critical questions are asked and answered without reserve, and yet—or perhaps I should say, in consequence of this—there is no trace of friction or of ill-humour, and no word of discourtesy is to be found in the whole correspondence.

Darwin's letters to my father, from the year 1845 till within a few weeks of his death in 1882, lie before me now, bound in a stout volume; and when I spoke about them to a distinguished man of letters a few days ago, he said: "If you can find, in all those letters, one containing an ill-natured remark, it is worth a considerable sum of money."

It very often falls to the lot of a publisher not only to superintend the more or less mechanical processes of producing a book, but also to take a considerable part in the editing, for which he may or may not receive thanks, according to the temperament and courtesy of the author. In the case of Darwin's works my father's part was mainly confined to the mechanical processes, and yet at every stage there came the kindly, considerate, never-forgotten word of thanks; and those who have smarted under the occasional omission of such courtesies can most highly appreciate the ready and ungrudging concession of them.

My father was introduced to Darwin by Sir Charles Lyell,

who was a personal friend of both and whose books he had published, and the introduction led to business relations in the following manner.

Darwin had originally brought out his "Journal and Researches" in the year 1840 in conjunction with the "Narratives of the Surveying Voyages of the *Adventure* and *Beagle*," by Admiral Fitzroy and Captain Philip King. Colburn was the publisher, but he only had the rights for the joint work, and as Darwin had in 1845 received no remuneration for his share of it, he was anxious to publish his journal separately, and came to consult my father, who at once agreed to include it in his "Home and Colonial Library."

Darwin writes to him :

I write now to ask whether you would so far greatly oblige me as to negotiate with Mr. Colburn, which I particularly dislike and should do badly. If you have any great objection I must write myself, and I would take the liberty of showing you my letter. . . . As I have never received one penny from Mr. Colburn, I have some claim on him, and I think mere shame would prevent him being rigid with me, though that is a weak hold on such a man.

This is the nearest approach to a "hard saying" that I have discovered among all the letters, and yet it is one which can hardly be brought within that definition.

The *Journal* was accordingly published in 1845, forming three parts of the Home and Colonial Series, and in about three years 7,000 copies were sold. Even the presentation of the customary "author's copies" called forth the following kind reply :

I must write to thank you for your really magnificent present of the twelve copies. I assure you I think I have not the smallest claims for them, after your other liberality.

Fourteen years now elapsed before Darwin again approached my father about the publication of a book ; but in March 1859 he consulted him about a work which he had just completed. So greatly was my father impressed by the importance of this proposal, that he wrote on April 1 :

I have no hesitation in swerving from my usual routine and in stating at once, even without seeing the MS., that I shall be most happy to publish it on the same terms as Sir Charles Lyell's books.

This book was the famous *Origin of Species*; but Darwin was not satisfied with a mere blind acceptance, and on April 5 wrote :

I send by this post the first 3 chapters. If you have patience to read all chap. I., I honestly think that you will have a fair notion of the whole book. It may be conceit, but I believe the subject will interest the public, and I am sure that the views are original. If you think otherwise, I must repeat my request that you will freely reject my work. I shall be a little disappointed ; I shall be in no way injured.

This note of modest confidence is struck in regard to all his works up to the last, and I have often heard my father quote it as an admirable contrast to the young author who " knows that his book is bound to make a sensation " at once, and sell by tens of thousands ; and I was reminded of it last week, when a writer offered me a pamphlet on an historical subject " which was sure to sell by millions very quickly."

By June the revision of proofs was in full swing, and Darwin wrote :

I remember writing to you that I thought there would be not much corrections. I honestly wrote what I thought, but was most grievously mistaken. I find the style incredibly bad, and most difficult to make clear and smooth. I am extremely sorry to say, on account of expense and loss of time for me, that the corrections are VERY heavy—as heavy as possible. . . . How I can have written so badly is inconceivable.

My father wrote some encouraging words, and asked to be allowed to show the proofs to Sir Charles Lyell. The reply was :

I am quite delighted at all you say and propose. Pray send the sheets to Lyell. Unfortunately, the part most likely to be interesting to him is not ready. . . . With hearty thanks, etc.

The cost of corrections certainly did prove unusually heavy, and, on sending the author an early copy, my father reported the matter, but expressed his intention of bearing the whole cost and charging no part of it to the author. The reply came in due course :

I have received your kind note and the copy. I am infinitely pleased and proud at the appearance of my child. You are really too generous to me about the scandalously heavy corrections. Are you not really acting unjustly towards yourself?

In those days, when fewer books were published and the average life of those that did appear was longer than at present, it was the general custom for each of the leading publishers to hold a "Sale Dinner" in November, to which all the principal booksellers were invited. The new books of the season were then introduced to them, and offered on specially favourable terms, to enable them to purchase stock for the ensuing six months or so. Such methods of business are impossible in these days of high pressure; and the Coffee-house Sales have almost passed out of memory.

In November 1859 the *Origin of Species* was thus first offered to the trade, and orders for 1,493 copies were there and then received. The first edition consisted of 1,250 copies (a large number for a new book of a comparatively little known writer in those days), and consequently a reprint had to be ordered at once.

On November 24, 1859, Darwin writes: "I am astounded at your news of the sale," and immediately set to work to make a few necessary corrections.

On December 5, 1859, he adds:

I have made some few corrections, and have inserted a capital sentence from the Rev. C. Kingsley in answer to any one who may, as many will, say that my book is irreligious.

P.S.—I have omitted what I particularly wished to say, namely to thank you sincerely for the exertions which you must have taken to sell my volume so promptly. It will have obviously an important effect in getting my notions known.

In July 1860 appeared the, now famous, article in the *Quarterly*, by Samuel Wilberforce, on the *Origin of Species*; and the author's first comment on it was:

The article on the *Origin* seems to me very clever, and I am quizzed splendidly. I really believe that I enjoyed it as much as if I had not been the unfortunate butt. There is hardly any malice in it, which is wonderful considering the source whence many of the suggestions come.

The reputation of the *Origin* was now firmly established in spite of the review in the *Edinburgh*, in which "I am thrashed in every possible way to the full extent of my bitterest opposing," and it is unnecessary for me to enter into any further record of new and amended editions, which were called for in rapid succession, as all this is recorded in the *Life and Letters*. Nor

will I dwell on the facts concerning the publication of the *Climbing Plants* and the *Variation of Animals and Plants under Domestication*, except to note that each was accompanied by the same modest appreciation of his own work on the part of the author, and the same courtesy in approaching his publisher.

We now come to the *Descent of Man*. Incomplete copies of this work were shown at the Sale Dinner in 1870, and 1,939 were ordered by the trade, but the book was not ready for publication till March 1871. The first edition of 2,500, as well as two further editions of 2,000 each, were sold out before the end of that year.

Capital review in the *Saturday R.* (writes Darwin), good notice in *Pall Mall*, and contemptuous as usual in *Athenæum*; but how dull and ignorant a man the writer of the last article must be!

In March 1871 he was "astonished at the sale of his book."

The *Expression of the Emotions* depended more than any of the other books on the illustrations; but in 1872 the art of photographic reproduction was in its infancy, and the greatest difficulty was experienced in obtaining good impressions of the pictures. Had the facilities which now exist been available then, the book might have been greatly improved, but the original photographs have been lost or have perished, and the work remains as it first appeared. On November 11, 1872, we read, "I am quite delighted, and more astonished than you can be, at the sale of the Expression book." Seven thousand copies were sold in the first year.

A reference to one more book shall be my last. One day, early in 1881, I was with my father in his room, when Mr. Darwin came in, and after some conversation, said: "Mr. Murray, I have brought you another book. It represents a good many years of hard work, and has proved of great interest to me, but I doubt very much whether it will interest the public, as the subject is not an attractive one. It is Earthworms."

Of this book 3,079 copies were sold at the dinner in November, and within a year seven editions had been called for.

I was told a few days ago by a friend, that he was in a provincial town in England when Darwin's name was mentioned. "Oh," said a local man who was present, "is not he the author who writes about Earthworms?" This would seem to show

that the book so modestly introduced had reached quarters into which Darwin's world-wide fame had not penetrated.

Incidentally, Darwin's works, and especially the *Origin of Species*, afford a telling illustration of one of the anomalies of our copyright law. The copyright of the first edition of the *Origin of Species* expired in 1901, and any one was then at liberty to reprint this edition. But Darwin himself had expressly said that he regarded it as imperfect, and did not wish it to be taken as representing his views.

The later editions, with their successive corrections and improvements, form in many respects a different book, and yet the imperfect book is being offered to the public without any clear statement of this discrepancy. This is perfectly legal, and no one can complain; but as a question of ethics it is somewhat difficult to solve.

I hope, if a new Copyright Bill is ever drafted, that, in justice to distinguished authors, this point will receive full consideration.

The system on which Mr. Darwin preferred to be remunerated for his books was to have an estimate made of each edition as it was printed, and to have his share of prospective profits paid him in anticipation. This is an unusual method of payment, and one which involves obvious disadvantages, if for no other reason because no one can tell exactly beforehand what an edition will produce, as there are several unknown factors in the calculation.

In the case of a disagreeable or suspicious man such a system would open the door to endless bickerings and friction; but though Mr. Darwin constantly inquired into details—whether of these estimates of payment, or of quality of paper or bindings, or other questions connected with the publication of his books—no angry or irritable word ever seems to have passed between him and his publisher. He did not blindly accept facts and figures which came before him; he investigated them all, and questioned where he was in doubt; but his questioning was always that of frankness and courtesy.

O si sic Omnes!



JOHN MURRAY.

1808—1892.

THE EXPECTED RETURN OF HALLEY'S COMET

By ANDREW C. D. CROMMELIN, B.A., F.R.A.S.

It is difficult for us now to realise the feelings of terror and dismay that the appearance of a large comet excited in the minds of men. Before the laws of gravitation were discovered, it was impossible to form any true conception of their distance or motion; from their great size and rapid motion, it was natural to assume that they were in near proximity to our earth—perhaps actually within her atmosphere—so that it was a simple step to associate their coming with plague, famine, and war. Even in the nineteenth century, comet-panics have not been unknown. An anticipated collision with Biela's comet, and the flaming visitants of 1858 and 1874 all caused lively apprehensions among those who had inaccurate ideas of their nature and their great distance from the earth. One reason of this apprehension is doubtless that bright comets generally form an exception to the astronomer's power of accurate prediction. They arrive unexpectedly, at quite irregular intervals, their periods being, as a rule, so long that they have not visited the sun's neighbourhood before during the historic period. Thus, there are sometimes several bright comets within a decade; but at the present time we have to go back to 1882 for a really brilliant one visible in Europe. There is just one exception to this inability to predict the advent of conspicuous comets, and that is in the case of Halley's comet. The class of periodic comets is indeed a large and increasing one, but most of them are only of interest to the professional astronomer, being faint or invisible to the naked eye. This comet is furthermore of interest from the very long list of observed returns that we are able to trace, which go back to the year B.C. 240.

In a retrospect of its history it is convenient to begin with

the time when its periodicity was first detected by the great astronomer whose name it bears, and to note that Newton also played an important part in this discovery. About 1679 he showed that a body moving under a central force inversely proportional to the square of the distance would describe an orbit in the form of one of the curves called conic sections, which are four in number—circle, ellipse, parabola, and hyperbola.

The planets and satellites gave examples of motion in very slightly flattened ellipses, some of them practically circles. It quickly occurred to him that the curious motions of comets might be explained by the hypothesis of their moving round the sun in very long ellipses, or, possibly, in parabolas. Halley entered into these ideas with enthusiasm, and his assistance was invaluable to Newton, who, in spite of his surpassing genius for abstract reasoning of the most profound character, seems not to have been equally successful with long arithmetical computations. Between them they studied the motion of the remarkable comet of 1680, which passed extremely near the sun. They found that its path differed very little from a parabola, but was perhaps a very long ellipse, with a period of some six centuries. It has to be remembered that we only see a very small portion of the whole orbit of the comet, since it soon vanishes from sight as it recedes from the sun. Even with the accurate observations that are now made, it is frequently difficult to determine whether an orbit is elliptical or parabolic; and the latter assumption is always made in the study of the early observations of a new comet, since all parabolas are similar curves, so that tables of parabolic motion can be constructed which are available for all cases, while ellipses are of all degrees of eccentricity, and each case has to be treated separately.

In 1682 appeared the object that was destined to make the name of Halley so famous; he was not, as some imagine, the actual discoverer—indeed, it is doubtful who saw it first. We learn from the record of Flamsteed, the first Astronomer Royal, that the object was seen by some neighbours on August 15 (Old Style), by his assistant on the next night, while he himself could not obtain an observation till the 20th. Halley was not then at the Royal Observatory; but we learn that he saw the

comet on at least one night, though clouds prevented him from measuring its place among the stars. It was not, however, this casual glimpse that entitled him to attach his name to the comet, but the remarkable researches, undertaken many years later, in which he found that it is a regular member of the sun's family, returning at intervals of about three-quarters of a century. This was a quite unexpected result of the immense piece of work which he set himself early in the eighteenth century—the calculation of parabolic orbits for which he could find sufficiently accurate observations—his researches including a period of fully two centuries. This was in those days a huge undertaking, for the most suitable methods had not yet been learnt by experience, and he had to devise all the needful machinery for himself as he went along. It must be acknowledged that there was some good fortune (though it was thoroughly well deserved) in the fact that the same comet should occur three times in his list of twenty-four comets. Halley was at once struck with the almost perfect similarity in the elements of their orbits; the solution of the mystery was not long in coming to him. He saw that it was in all probability the same object returning at regular intervals to the sun—that, to quote his own expression, here was “A Mercury among comets”—comparing the planet of shortest period with what he supposed, though wrongly, to be the comet of shortest period, and drawing the deduction, which was natural, though we now know it to be false, that, as this one had come back, so all the comets would return in longer or shorter periods. We now know a great number of comets with shorter periods than that of Halley, the one that is the true Mercury of comets being Encke's, which returns to perihelion thirty times in a century.

There was just one feature that made Halley hesitate before definitely announcing his discovery. The three dates that he found for the comet passing nearest to the sun were August 24, 1531; October 16, 1607; September 4, 1682; the first interval exceeds the second by fifteen months. Now, the periods of the planets are practically the same, revolution after revolution; but the true explanation was soon seen. He remembered that there was a sensible, though small, variation in the periods of the giant planets, Jupiter and Saturn, due to their mutual attractions, and that not only did the comet pass much nearer to them than

they do to each other, but also that, in the distant regions of space to which it travels, a small change of velocity would have an exaggerated effect on the period. We now know that the effect of these planets can be even greater than that necessary to explain Halley's fifteen months, and that the total range is five years, from seventy-four years to seventy-nine. Halley then found that a similar comet had appeared in 1456, which he had little doubt was the same object. So confident did he now feel, that he ventured to predict the return of the comet in 1758; subsequently he made this a year later, having found that Jupiter's action would delay it. He added that, "If the comet returns in accordance with my prediction, posterity will not forget that this first prediction was made by an Englishman." When the time approached, the French mathematicians Clairaut and Lalande made calculations to fix the date of the return as accurately as possible. They received material assistance from a lady, Madame Lepaute, in this huge undertaking, which involved a study of the comet's motion for two revolutions—viz. from 1607 to 1682—in order to find the rate at which the comet was moving in 1682, and from that year to 1759 in order to find how much this rate was altered in the interval. They just finished their work in time, announcing in November, 1758, that the comet would pass its perihelion (or nearest point to the sun) on April 13 following. It was actually found by Palitsch on Christmas Day, and passed perihelion just a month earlier than the time predicted, this being a small discordance when it is considered that the masses of the planets were still imperfectly known, and that Uranus and Neptune had not yet been discovered.

It is a curious illustration of the dictum that "A prophet has no honour in his own country," that, while the French astronomers thus triumphantly vindicated Halley's prediction, in England the *Gentleman's Magazine* expressed grave doubt as to whether the comet would reappear at all, and referred to Halley somewhat contemptuously. However, after the comet was seen, it changed its tone, and published a poem, from which these lines are taken :

Hah ! there it flames, the long-expected star,
And darts its awful glories from afar !
Punctual at length the traveller appears,
From its long journey of near fourscore years.

Lo! the reputed messenger of fate,
 Arrayed in glorious but tremendous state,
 Moves on majestic o'er the heavenly plane,
 And shakes forth sparkles from its fiery train.

Ye sons of science, from your high abodes,
 Descry its oblique path, and mark its nodes,
 Explore with what velocity 'tis hurled,
 And how exact its period round the world ;
 Now, now, in this delightful work engage.
 Pursue the steps of the sagacious sage,
 And be this wiser than the former age.

The apparition of 1759 was not very remarkable for its brilliance. The comet was, however, distinctly visible to the naked eye, and had a long thin tail.

Before it returned again in 1835, the science of perturbations had made great strides. Many mathematicians engaged in this work, the most successful being Rosenberger and de Pontécoulant, who both predicted the time of perihelion correctly within a very few days. The work of the former was on more rigorous lines, and he took account of a few minute disturbances that were neglected by the other.

The comet was first seen by M. Dumouchel, Director of the Observatory of the Roman College, on August 5, 1835. It passed its perihelion on November 16, and remained in sight till the following May, its changes of appearance being carefully followed by astronomers all over the world. Though not equal in splendour to the greatest comets of the nineteenth century, such as Donati's in 1858, or those of 1811, 1843, 1882, it was nevertheless an extremely interesting object, from the rapid changes which it underwent from night to night. In this respect it seems to have closely resembled comet Morehouse, which has just passed out of reach of Northern observers, but is still under observation in the Southern Hémisphere. The nucleus in each case appeared to be in a state of great activity, and ejected streams of matter in various directions with a violence that has been compared to a volcanic outburst. These streams are at once repelled by the sun, the repulsive force being either of an electrical nature or being simply the action of light on very finely divided matter. The main tail is directed almost exactly away from the sun, but there are a number of lateral tails or brushes, making various angles with this. It

was shown by Bessel that some of the changes could be explained by supposing that the comet was rotating in about five days, and a similar suggestion was made for comet Morehouse. A difficulty arises in supposing a body to rotate that is neither a rigid body nor held together by a strong central force. However, it seems that a rotating head would produce a semblance of rotation in the tail streaming from it, although the separate particles of the tail are not rotating, but simply being driven outwards.

We now enjoy a great advantage over the observers of 1835 in being able to apply photography to the study of comets' tails. This was first rendered possible in 1881 by the invention of the dry plate, the wet plate formerly in use being too slow for such faint objects. An almost continuous series of photographs was obtained of comet Morehouse, through the co-operation of several observatories, and it was possible to trace the course and speed of the several portions of the tail, as they were repelled from the head and from the sun. The repulsive force is found to be considerably stronger than gravitation, so that, obviously, the matter driven off is lost to the comet, which must therefore undergo a continuous loss of substance. It is not, therefore, difficult to understand that Halley's comet has appeared much less brilliant at recent returns than it seems to have been several centuries ago, when it caused widespread terror.

Another feature that Halley's comet in 1835 shared with comet Morehouse was the fact that both, at times, lost their tails for a short period. Since we may be sure that the sun's repulsive action did not cease, we must suppose that this arose from some change in the head of the comet, the nature of which we cannot guess. Comet Morehouse seemed to have a fairly regular cycle of changes, so that it was even possible to predict when the tail was likely to disappear, and when a new outburst was likely to occur.

Before looking forward to the return of Halley's comet next year, I propose to carry its story backwards as far as we can go. Halley himself traced it as far back as 1456, and indeed thought that he had traced it some fifteen hundred years further, but he made an error in his identification before 1456, which affects all the returns before that. Laugier was the first to carry it one step further, showing that the comet observed in China in 1378

was undoubtedly Halley's. Hind contributed a valuable paper to *Monthly Notices of the Royal Astronomical Society*, vol. x., in which he carried back the history of the comet, with some probability, to the year 12 B.C. He did not compute the perturbations, but based his conclusions on the apparent motions of the comet.

In the last two years the calculation of the perturbations which the planets have produced on the comet's motion has been carried back to the beginning of Hind's list, and even further; the following list is the result of this examination, and all the returns given in it may be accepted with certainty except the first, which is somewhat doubtful. Hind's list was only sensibly in error in four cases—viz., he gave

A.D.	608 Nov.	instead of	607 March.
„	912 April 1	„ „	912 July 19.
„	837 April 6	„ „	837 Feb. 25.
„	1223 July	„ „	1222 Sept.

It should be added that, owing to the vagueness of the observations, the earlier dates are uncertain by a fortnight or more; as the observations become more precise the uncertainty diminishes, so that for recent returns it is a very small fraction of a day.

The list on next page gives the dates of perihelion, the same expressed in Julian days (a very useful way of giving them, since it gets rid of the alterations of the calendar and of the change from B.C. to A.D.), and a few brief notes. Fuller notes on some of the more interesting returns are given at the end.

It will be seen what an important part the Chinese records have played in the preservation of the earlier apparitions of the comet. Furthermore, while the Western observers generally gave very meagre particulars as to the part of the sky where the comet was seen, the Eastern ones give precise descriptions of its path through the constellations. It is likely that their care in this respect was not altogether disinterested, since they imagined that the terrestrial kingdoms had their counterparts in the sky, and that the comets were ambassadors between them, indicating corresponding relations between the kingdoms

No.	Date of perihelion.	Julian day.	Interval in days.	Where seen.	Notes.
1	B.C. 240, May 15	1,633,898	28,129	China	Identification doubtful.
2	B.C. 163, May 20	1,662,027	27,846	—	{ No record: from calculation only.
3	B.C. 87, Aug. 15	1,689,873	27,448	China	—
4	B.C. 12, Oct. 8	1,717,321	27,869	China, Rome	{ Very fully described by Chinese.
5	A.D. 66, Jan. 26	1,745,190	27,817	China	—
6	A.D. 141, Mar. 25	1,773,007	27,771	China	—
7	A.D. 218, Apr. 6	1,800,778	28,125	China, Rome	{ "Fearful, flaming star." Preceded death of Emperor Macrinus.
8	A.D. 295, Apr. 7	1,828,903	28,704	China	—
9	A.D. 373, Nov. 7	1,857,607	28,362	China	—
10	A.D. 451, July 3	1,885,969.5	28,990	China, Rome	Preceded death of Attila.
11	A.D. 530, Nov. 15	1,914,959	27,890	China, Europe	Called Lampadias.
12	A.D. 607, Mar. 26	1,942,849	28,370	China	—
13	A.D. 684, Nov. 26	1,971,219	27,591	China, Japan	{ Drawing made at Nuremburg.
14	A.D. 760, June 10	1,998,810	28,018	China	—
15	A.D. 837, Feb. 25	2,026,828	27,538	China	{ Preceded death of Louis le Debonnaire.
16	A.D. 912, July 19	2,054,366	28,182	—	{ No record: from calculation only.
17	A.D. 989, Sept. 15	2,082,548	27,950	China	—
18	A.D. 1066, Mar. 25	2,110,498	28,880	Europe, China	{ Norman conquest, Bayeux tapestry.
19	A.D. 1145, Apr. 19	2,139,378	28,268	China	—
20	A.D. 1222, Sept. 10	2,167,646	28,898	Europe, China	—
21	A.D. 1301, Oct. 22.7	2,196,543.70	28,141.07	Europe, China	—
22	A.D. 1378, Nov. 8.77	2,224,684.77	28,336.44	China	—
23	A.D. 1456, June 8.21	2,253,021.21	27,471.59	Europe, China	War against Turks.
24	A.D. 1531, Aug. 25.8	2,280,492.80	27,811.07	Europe	{ First return discussed by Halley.
25	A.D. 1607, Oct. 26.87	2,308,303.87	27,351.94	Europe	{ Second return discussed by Halley.
26	A.D. 1682, Sept. 14.81	2,335,655.81	27,936.76	Europe	Periodicity discovered.
27	A.D. 1759, Mar. 12.57	2,363,592.57	28,006.37	{ Throughout world	{ First predicted return.
28	A.D. 1835, Nov. 15.94	2,391,598.94	27,180	{ Throughout world	{ Second predicted return.
29	A.D. 1910, Apr. 17	2,418,779		—	{ Some give dates a month or two later.

on earth, so that valuable political information was to be gained from a study of the heavens. We see the occurrence of a similar idea in the West in the many cases where they have recorded that a comet appeared before the death of a king, or other striking event. Josephus noted that a great curved comet was seen for several months, not long before the fall of Jerusalem; it is not unlikely that this was the apparition of A.D. 66. Similarly for the deaths of Macrinus, Attila, and Louis le Debonnaire. Up to the apparition of 1378 inclusive the Chinese records of position are superior to the European ones; the

Chinese have, however, stood still, while the accuracy of Western observations has increased in a marvellous manner, so that now there is no comparison possible between the two.

The apparitions of 1066 and 1456 are of such historical interest that they deserve fuller treatment than the others.

The comet was discovered in China on April 2, 1066, when it was a morning object in Pegasus, and rapidly approaching the sun, which it passed about a fortnight later, becoming an evening object about April 24 (a week after Easter). It was then first seen in Europe, and is described as a star without rays, resembling a white cloud, 3° in diameter. It must then have become very brilliant, being close to the earth, and traversed the constellations of Gemini, Cancer, and Hydra, being finally lost to view in Crater, after a period of visibility of about two months. Pingré, the author of the famous *Cometographie*, has the following passage on the effect described in Europe by this comet: "It was regarded as a presage of the conquest of England by William, Duke of Normandy. This gave rise to some verses. . . . According to a poet of that time, the comet had been more favourable to William than Nature to Cæsar; the latter had no hair, William received some from the comet:

'Cæsariem, Cæsar, tibi si Natura negavit,
Hanc, Willelme, tibi stella Comata dedit.'

Was this 'hair' the crown of England? A monk of Malmesbury, seeing his country on the point of being attacked, on the one side by Harold, King of Norway, on the other by William, and concluding that blood would be shed, addressed the comet thus: 'I see you then, origin of the tears of many mothers; I have seen you for long, but now you appear more terrible; you threaten my country with entire ruin.'

The situation is vividly portrayed on the Bayeux tapestry, where a group of Normans are shown excitedly gazing at the comet, which is shown above them with a long, flaming tail, and the legend, "Isti mirantur stellam." In the next compartment a messenger announces to Harold the advent of the comet and of the Norsemen, whose ships are shown below. Harold is in great agitation at the combination of misfortunes, and

appears to totter on his throne. William appears to have reassured his courtiers, who were also anxious, by saying that such a comet only appeared when a kingdom wanted a king!

In 1456 Europe was in a state of great excitement. Constantinople had recently fallen into the hands of the Turks, who were now besieging Belgrade under Mahomet II., and it was feared that if it fell nothing would resist their victorious march. To quote an article by Mr. E. V. Howard: "The two opposing armies in open sight are resting from their conflict, for the dark shades of night are falling on the earth. . . . Suddenly the peaceful meditations of the sentinel are disturbed; he is startled and perplexed, for in the western sky a comet appears, with a long dragon-like tail, which spreads over two celestial signs, and is apparently advancing towards the moon. His fears are heightened as a dark shadow steals over the moon's face, and hides its light."

Pontanus continues the narrative: "Some persons, seeing the darkness of the eclipse, and perceiving the comet in the form of a long sword advancing from the west, and approaching the moon, thought that it presaged that the Christian inhabitants of the West would come to an agreement to march against the Turks, and would gain the victory. And the Turks, on their part, taking into consideration the state of affairs, fell into no small fears, and entered into serious arguments as to the will of Allah."

A large number of astronomical text-books contain a statement that Pope Calixtus III. cursed and excommunicated this comet, and ordered prayers to be said to ward off the evil influences of "the Devil, the Turks, and the Comet." Father Gerard, in a recent article in *The Month*, has shown that this is a pure legend, the only shadow of authority for it being in one writer, Platina, who says that the most learned men of the day predicted that the comet would produce pestilence. This was in full accord with the ideas of that time, it being supposed that comets were deleterious vapours in the upper air. Platina then says that the Pope, on hearing these opinions (without himself endorsing them), said that, if such evils were indeed threatened, it would be well to pray that the Christian army might be spared from them. The whole story of Platina, however, carries very little weight, since he does not give any

authority for it, and the bulls of Calixtus III. are extant, and contain no reference to the comet.

On referring back to the list of returns, it will be seen that the revolution now being accomplished is much the shortest on record, while that between 451 and 530 is the longest, the difference being almost five years. The differences between the lengths follow a kind of sequence, as was first detected by Angstrom in 1862. To see this it is advisable to consider the aphelion passages, as some minor irregularities are thus practically eliminated, which would affect the results if we took the perihelion passages.

In the following list the dates are all expressed in Old Style, and the B.C. dates are measured in the negative direction from the beginning of the year 0. The three dates marked with an asterisk are subject to a little uncertainty.

No. for reference.	Aphelion passage.	Normal date.	First minus second.	Angstrom term.	Residual.
			y	y	y
1	— 200'21*	— 199'39	— 0'82	+ 2'06	— 2'88
2	— 123'57*	— 122'62	— 0'95	+ 1'76	— 2'71
3	— 47'78	— 45'86	— 1'92	+ 0'85	— 2'77
4	+ 27'93	+ 30'91	— 2'98	— 0'36	— 2'62
5	103'61	107'68	— 4'07	— 1'45	— 2'62
6	179'75	184'45	— 4'70	— 2'03	— 2'67
7	256'75	261'22	— 4'47	— 1'90	— 2'57
8	334'55	337'99	— 3'44	— 1'12	— 2'32
9	412'67	414'76	— 2'09	+ 0'05	— 2'15
10	491'17	491'52	— 0'35	+ 1'21	— 1'56
11	569'03	568'29	+ 0'74	+ 1'94	— 1'20
12	646'02	645'06	+ 0'96	+ 2'00	— 1'04
13	722'62	721'83	+ 0'79	+ 1'36	— 0'57
14	798'80	798'60	+ 0'20	+ 0'25	— 0'05
15	874'86	875'37	— 0'51	— 0'95	+ 0'44
16	951'14	952'14	— 1'00	— 1'82	+ 0'82
17	1027'98	1028'91	— 0'93	— 2'06	+ 1'13
18	1105'77	1105'68	+ 0'09	— 1'58	+ 1'67
19	1184'00	1182'45	+ 1'55	— 0'56	+ 2'11
20	1262'25	1259'22	+ 3'03	+ 0'66	+ 2'37
21	1340'33	1335'99	+ 4'34	+ 1'65	+ 2'69
22	1417'65	1412'76	+ 4'89	+ 2'06	+ 2'83
23	1494'04	1489'52	+ 4'52	+ 1'76	+ 2'76
24	1569'72	1566'29	+ 3'43	+ 0'85	+ 2'58
25	1645'23	1643'06	+ 2'17	— 0'36	+ 2'53
26	1720'92	1719'83	+ 1'09	— 1'45	+ 2'54
27	1797'50	1796'60	+ 0'90	— 2'03	+ 2'93
28	1873'05	1873'37	— 0'32	— 1'90	+ 1'58
29	+ 1948'20*	+ 1950'14	— 1'94	— 1'12	— 0'82

The normal date No. 16 is the mean of all the observed dates from Nos. 3 to 29 inclusive. The other normal dates are found by the successive application of the average period, 76.77 years. The column "First—second" shows the discordance of the actual aphelion passage from the normal date. These are plotted in a diagram, and it is readily seen that more than one wave is indicated. There is one pretty regular wave (discovered by Angstrom) with a period of $10\frac{1}{2}$ revolutions of the comet, and an amplitude of about 2.07 years. This is tabulated above, and also the residual after the Angstrom term has been removed. Angstrom considered that the residuals formed another regular wave with period 2,650 years. This is not borne out by the longer and more exact series of returns of the comet that is now available. Some part of it may form a wave, but it seems not unlikely that part of it is a parabolic curve, which would indicate an acceleration of the comet's motion, such as has already been detected in the case of Encke's comet.

This may arise from a resisting medium. The effect of such a medium is not to lengthen the period, as might be thought; for it causes the comet to approach the sun, and the effect of this more than counterbalances the diminished rate of motion. Another cause that may affect the period is the alteration of mass produced by the matter driven off in the tail when the comet is near the sun. It should be borne in mind that the effect of all non-gravitational causes on the period cannot amount to more than a day or two in each revolution. Thus it is difficult or impossible to make calculations sufficiently exact to detect their effect in modern times; but, since the effect of an acceleration increases as the square of the time, even a small acceleration produces sensible effects in the course of 29 revolutions. We have an exactly similar case in the acceleration of the moon's motion; this cannot be found even from all the exact observations made in modern times, and yet the rough observations of ancient eclipses are of use in its determination.

We now propose to trace the movements of the comet since its last visit. One year after perihelion it crossed the orbit of Jupiter, and five months later made its nearest approach to that planet, the distance being 2.7 astronomical units. It had made a still nearer approach to the giant planet (1.3 units) nine-and-a-half months before perihelion. It was these com-

bined approaches that had such a notable effect on the period. The orbit of Saturn was crossed in February 1839, that of Uranus just five years later; the orbit of Neptune, the most distant known planet, 30 units from the sun, was reached in December 1856. For sixteen more years the comet receded, reaching its greatest distance, 35 units, early in 1873. It then commenced its return journey, again crossing Neptune's orbit in April 1889. Thus nearly half the periodic time is expended in covering the small arc lying outside Neptune's orbit. The similar arc in the sun's neighbourhood is covered in two years, which gives a vivid idea of the great change in the speed. The orbit of Uranus was crossed in March 1902, that of Saturn in February 1907. Jupiter's orbit will be crossed in April of this year, but the comet has already made its nearest approach to that planet, which on this occasion is 5 units.

We come now to the question, When is it likely to be visible (i) with the telescope, (ii) with the naked eye? In 1759 it was seen from 77 days before perihelion to 102 days after; in 1835 from 102 days before to 185 days after. It must be remembered that not only are large telescopes more numerous than formerly, but photography has given us the power of recording intensely faint objects, that would be wholly invisible even in the largest telescopes. Consequently, it was thought not unlikely that the comet might be photographed even some months ago; however, no certain trace of it has yet been found on the plates, though there are some suspicious objects awaiting verification. If not found by mid-March we must wait till the autumn, as the region of Orion, where it now is, gets too near the sun to permit of long-exposure photographs. It is almost a certainty that it will then be found. Dr. Smart has prepared an ephemeris for its motion in 1910, a copy of which is given. It is based on the supposition that the perihelion passage will be on April 16, 1910, a date which I consider the most probable, though it is right to add that some authorities give dates as much as two months later. It will be nearly stationary in Pisces in January and February, and probably visible with small telescopes, possibly even with the naked eye. It will then pass behind the sun, being a fairly bright morning-star in April, and the beginning of May. It will again pass the sun on May 17, and for a few days after this may be expected to be at its greatest splendour, being only 12,000,000 miles from the earth. In

consequence of this near approach, its motion will be very rapid, and it will pass in a few days from Taurus to Hydra, and Sextans.

It will, unfortunately, be south of the sun, which will greatly impair its splendour as seen from England, though in the Southern Hemisphere it is likely to make a good display. The circumstances as regards its position in the sky are very like those in 1066, when it made such a sensation; but in all probability the comet has lost much of its glory in the interval, so that not too much should be expected next year, though we are quite justified in hoping for a fairly conspicuous object with a tail several degrees in length, probably in a state of rapid change, as in 1835; and with the aid of the spectroscope and the camera we may hope to interpret these changes much more fully than was possible then. But, apart from its intrinsic splendour, the comet has such an interesting history, and links the centuries together so wonderfully that no one gifted with the smallest degree of imagination can fail to follow its course with the deepest interest, and renewed admiration for the great English astronomer whose name it bears, and who discovered its periodicity in such a brilliant manner.

THE EXPECTED RETURN OF HALLEY'S COMET 557

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EPHEMERIS OF HALLEY'S COMET IN 1910, BY DR. SMART.

It is for Greenwich midnight. Δ is the distance from the Earth in astronomical units.

Date.	R.A.	N. Dec.	Δ	Date.	R.A.	Dec.	Δ
1910:	h m s	10° ' "		1910:	h m s	8° ' " N.	
Jan. 6	1 51 40	10 17	1'42	May 8	0 12 16	8 43	0'41
" 10	1 39 44	9 43	1'46	" 9	0 19 52	9 16	0'37
" 14	1 29 8	9 15	1'50	" 10	0 29 8	9 48	0'33
" 18	1 19 44	8 50	1'54	" 11	0 40 56	10 25	0'30
" 22	1 11 20	8 29	1'59	" 12	0 56 32	11 12	0'26
" 26	1 3 56	8 11	1'63	" 13	1 16 56	12 11	0'23
" 30	0 57 32	7 57	1'67	" 14	1 44 12	13 23	0'19
Feb. 3	0 51 48	7 45	1'71	" 15	2 28 44	14 25	0'16
" 7	0 46 36	7 37	1'75	" 16	3 10 56	16 2	0'15
" 11	0 41 56	7 31	1'78	" 17	4 13 56	15 50	0'14
" 15	0 37 36	7 27	1'81	" 18	5 23 52	15 41	0'13
" 19	0 33 44	7 24	1'84	" 19	6 30 16	13 38	0'14
" 23	0 30 8	7 22	1'85	" 20	7 23 20	11 11	0'16
" 27	0 26 44	7 22	1'86	" 21	8 2 40	8 55	0'19
Mar. 3	0 23 34	7 23	1'87	" 22	8 31 56	7 5	0'22
" 7	0 20 14	7 24	1'87				
" 11	0 16 56	7 24	1'86	" 26	9 31 28	2 53	0'36
" 15	0 13 40	7 25	1'83	" 30	9 56 16	1 0 N.	0'51
" 19	0 10 24	7 27	1'78	June 3	10 9 56	0 4 S.	0'66
" 23	0 6 48	7 26	1'75	" 7	10 19 48	0 43	0'81
" 27	0 3 0	7 23	1'71	" 11	10 27 28	1 15	0'97
" 31	23 58 52	7 17	1'64	" 15	10 31 56	1 42	1'12
Apr. 4	23 55 2	7 13	1'56	" 19	10 36 24	2 3	1'28
" 8	23 51 8	7 6	1'47	" 23	10 40 16	2 24	1'40
" 12	23 47 28	7 0	1'36	" 27	10 44 0	2 45	1'54
" 16	23 44 6	6 52	1'23	July 1	10 47 28	3 6	1'70
" 20	23 41 36	6 43	1'09	" 5	10 50 36	3 24	1'86
" 24	23 41 20	6 48	0'95	" 9	10 53 40	3 41	1'95
" 28	23 43 16	7 4	0'80	" 13	10 56 40	4 1	2'07
May 2	23 48 56	7 20	0'65	" 17	10 59 40	4 20	2'19
" 6	0 1 44	8 18	0'49	" 21	11 2 36	4 38	2'31
" 7	0 6 32	8 28	0'45	" 25	11 5 28	4 56 S.	2'43

CHLOROFORM IN USE

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	PAGE
INTRODUCTION	558
APPARATUS	559
THE CHLOROFORM CURVE	563
FOUR-FIFTHS OF MANKIND	565
THE EXCEPTIONS	568
CONCLUSIONS	571

INTRODUCTION

EXACTLY twelve months ago two important papers appeared in this journal. In the first, Waller (6 *a*) pointed out the much greater safety of anæsthesia when the percentage of CHCl_3 in the inspired air was known, and the need for the adoption of some such system in the case of the human subject. In the second, Buckmaster (8 *a*) gave a résumé of the recent physiological work on the action of chloroform in the body and the amount actually present in the blood in different stages of anæsthesia. It was plain that the time had come to apply the knowledge so acquired to the more difficult problem of anæsthesia in man. It is by no means the first time that this problem has been attacked. To mention only the more prominent names, Snow (1), Cushny (2), Dubois (3), Vernon-Harcourt (4), Levy (5), Waller (6 *a*, *b*), Roth, Hagen (13), and Kionka and Krönig (7 *b*) have all made observations on the percentage necessary for anæsthesia in the human subject, using apparatus giving a measured amount of the anæsthetic; and Victor Horsley (9) has published a curve, constructed on lines similar to those considered later, showing the percentages used in operations on the central nervous system. But if we bear in mind the recent advances in our knowledge of the subject, particularly the rapid and accurate methods introduced by Waller (6 *b*, *c*) for the estimation of chloroform vapour in air, it can easily be seen that the conditions at the present time are much more favourable for the construction and employ-

ment of accurately graduated apparatus. The following paper records the application of the methods first used on animals to the human subject; and it will be seen how closely, in spite of the difference in species, man approximated to the other mammalia, and how the canons of correct administration as determined on the latter apply to the human subject.

The limits, however, of the inquiry are very narrow. Anæsthesia with chloroform is only practised in man when an operation of some duration, and usually of some severity, is required, and the mere fact of an operation introduces new conditions which complicate the observations. The state of the patient must ever be borne in mind, and it is usually inadvisable for many reasons to keep the patient under as deeply as is the custom in the lower animals, so that here again the comparison is not easy. Further, the aim of the anæsthetist is, with human patients, to use as little chloroform as will suffice for the end in view, and this varies with every operation, some cases requiring the greatest relaxation obtainable with deep anæsthesia; others, requiring merely the abolition of pain, are given the smallest amount of the drug that is necessary for the purpose, as is well seen in Sir Victor Horsley's curve of his operations on the central nervous system. Such conditions are fixed by the necessities of each case, and all that can be done, when it is desired to study any given phenomenon, is to multiply the observations till a sufficient number of illustrative cases are encountered. In a word, it is necessary to substitute *observation* for *experiment*.

APPARATUS

The first point on which attention must be fixed is the apparatus which is to be used. It is not necessary to go into the merits and demerits of all the various forms of apparatus that have been mentioned;¹ but a few remarks on the "open" method of administering chloroform may not be out of place, as this, in spite of its drawbacks, must be taken as the standard of comparison with all the newer methods, on account of the large number of cases that are still anæsthetised by its use.

By the open method chloroform is dropped on a piece of lint or similar substance, and the evaporation from this gives a

¹ For an examination of the Harcourt, Brodie, Dubois, and Junker apparatus see Waller and Wells (6 *d*). On the Junker apparatus see Chapman (14).

greater or less percentage of vapour in the air, some of which is inhaled by the patient. Simplicity is the chief merit of this way of administering the drug. The amount of evaporation is governed by at least four factors of varying importance: the temperature of the air (*a*), the amount of chloroform which is dropped on the lint (*b*), the amount of water condensed on the lint from the patient's breath (*c*), and the texture of the lint (*d*). All these have to be taken into account if the strength of the vapour is to be regulated. Finally, the patient inhales the vapour from the cone mixed with a greater or less amount of air coming in from the sides of the mask, so that it is not surprising that the principal guide of the practical anæsthetist has hitherto been the condition of the patient rather than the strength of the vapour that he is supposed to be inhaling. Attempts have been made to determine the amount of chloroform given off from such a mask, but space does not permit of a full account of the difficulties met with in such determinations, and the reader must be referred to the works of A. G. Levy (5*d*) and W. Legge Symes (10) for information on this subject.

It is plain that, if a constant percentage of chloroform vapour could be attained in the air actually inspired by the patient, the sources of uncertainty just referred to would be removed. Very much would still remain to be learnt about anæsthesia, but at least one would know more than had been previously possible.

Two quite distinct methods are available for the construction of an apparatus intended to administer a known percentage of vapour: (*a*) the so-called "vacuum," and (*b*) the "plenum." The "vacuum" system presents some advantages, and has been employed by Snow, Vernon-Harcourt, and Levy. Here the patient by his respiratory muscles draws the air which he inspires over the surface of liquid chloroform, and this, evaporating to a greater or less degree according to circumstances, gives to the inspired air a definite amount of chloroform vapour. The good points of the system are that it is possible to construct a small and portable apparatus, and that, as all the chloroform evaporated is actually inspired, there is no waste. The chief drawback is that there is necessarily an additional tax placed on the patient's respiratory powers at a time when these are not in a condition that suggests the imposition of any extra load. How far this load is of any account is not very clear. At the

least, it introduces a new factor into a problem already sufficiently complex, and, chiefly for this reason, the second system, the "plenum," has been preferred for the present purpose.

In the "plenum" system the air is forced through the apparatus by some form of mechanical power. Dubois, Waller, and Roth-Dräger adopted this method, and supplied the air (in the latter case O_2) charged with chloroform vapour in a constant stream to the patient. One drawback to the method is obvious. It is not practicable to supply the mixture only during inspiration, and so at least one-half is wasted, as, during expiration, the mixture passes away unused. It might be possible to obviate this; but the saving of chloroform would hardly pay for the additional complication required, particularly as, even with this drawback, less chloroform is used than with the old open method. In other respects the plenum system presents distinct advantages.

The apparatus adopted in the present series of observations (Alcock, 7 *a*) was of a very simple construction. It consists of a circular copper vessel 5 in. in diameter and $4\frac{1}{2}$ in. deep, which contains 150 c.cm. of chloroform; $1\frac{1}{4}$ in. from the bottom is fixed a shelf closed except for two oblong holes. Immediately above and touching this shelf is a circular plate, movable by means of a hollow rod in the centre, and pierced by two triangular apertures. These can be adjusted by the centre rod so as to expose more or less of the fixed oblong holes in the shelf, and so produce greater or smaller openings into the space below. Air (supplied from a small foot bellows or from an electric fan) enters the chamber by one tube opposite one aperture, and leaves by another tube, taking up more or less chloroform vapour according to the size of the apertures. A thermometer in the hollow rod indicates the temperature of the chloroform below, and a water-jacket surrounding the chamber serves to keep the temperature between certain limits.

The graduation of such an instrument is a matter requiring a little care. The calculations of the percentages which the machine might be theoretically expected to yield are rather complex, and so it seemed advisable to graduate the machine by actual estimation. This involved the making of several hundred determinations of the percentages given under varying conditions, and it was essential, therefore, to possess a rapid and

accurate means of estimation. Fortunately, such a method has been introduced by Waller and Geets, and perfected by the former author. Levy (5 *d*) has shown that this method gives practically identical results with that of the combustion method of Vernon-Harcourt (4 *a*). Careful experiments by Waller's method showed that under the conditions of the present research the weights could be relied upon to within 1 milligramme—that is, the error of each estimation was less than 1 per cent. of chloroform.

Greater accuracy than this requires that rather elaborate precautions should be taken, so the arbitrary rule was made that the graduations of the machine should be correct only within this limit of error. It was found subsequently that a variation of less than 25 per cent. was quite inappreciable under the ordinary conditions of human anaesthesia, and so for the present purpose the limit of error of 1 per cent. gives quite sufficient accuracy.

A scale was then constructed, and was tested under all the conditions which could be expected to arise. It was found that many circumstances could be neglected in practice. For example, the level of chloroform has only a very slight influence on the percentage, so that, even after working twenty minutes, the percentage was practically unchanged. If the apparatus is accidentally shaken, this produces no change in the percentage. A small amount of water condensed from the air has an effect only within the limits of error, and variation in the air supply between 8 litres and 20 litres per minute is also without effect. This last result is of importance and was specially examined. Levy has already referred to the physical law on which this depends (1).

The effect of temperature remains to be considered. As the temperature rises more chloroform evaporates, and the percentage at the same opening increases. This temperature correction is greater with a high percentage than with a low. But the construction of the sectors is such that a given length of movement causes a greater increase in the percentage when the sectors are nearly open than when they are nearly shut, so that the graduations of the scale marking the percentages are closer together at the high values than at the low.

If, then, such a scale is made to slide bodily to right or left in accordance with the temperature, a given move-

ment (*e.g.* from between 65° and 70° F.) will indicate a greater correction at a high percentage, which is what is required from the construction of the apparatus. The scale could therefore be constructed to give a theoretically perfect correction. As a matter of fact the correction in the apparatus was within '1 per cent. of the truth between '5 per cent. and 3 per cent.; above and below these limits the error was a little greater than this ('2 per cent.), but, as the temperature correction for these extreme percentages is seldom required in practice, the elimination of this error was left for a future occasion.

From all these considerations, and others that will appear later, it is reasonable to conclude that the patients anæsthetised with the aid of this apparatus have had the actual percentage administered to them that was shown on the graduation of the instrument. This may seem to the scientific mind a truism, but in this the scientific mind would reason incorrectly, as the behaviour of a machine in the operating-theatre is too often not that which had been anticipated by the designers.

THE CHLOROFORM CURVE

The next step in the problem is to determine with what percentage the anæsthesia is to be carried out. Obviously, it is not sufficient merely to know what percentage is being given; the *known* percentage must also be the *correct* one, and, before this can be ascertained on the patient, there are several points to be considered, which may be briefly summarised in this place.

I. The percentage to be inhaled must not be above 2 per cent. at the beginning, else there will be coughing from the action of the concentrated vapour on the glottis.

II. Waller has shown that in animals a percentage much over 2 per cent. is directly dangerous.

III. Brodie and Widdows (11) state that the most rapid absorption of chloroform is to be found in the second minute of anæsthesia.

IV. Buckmaster and Gardner (8) find that there is a definite danger-point in the first few minutes of anæsthesia, due to the rapid rise of the chloroform contained in the blood.

V. Embley (12) points out very clearly the mechanism by means of which such danger arises.

All these five considerations negative the administration of a high initial percentage of chloroform. But on the other hand there is the factor of

VI. Time. The relation of the time taken to produce surgical anæsthesia and the percentage in the inspired air is a very curious one, and deserves further study than has been hitherto accorded to the subject. Neglecting for the moment individual variations, percentages below 2 per cent. only induce anæsthesia in the human subject if a quite unreasonable time is taken in the process; in round numbers twenty minutes to three-quarters of an hour may be required. This time serves no good purpose—in fact, certain observations now in progress appear to indicate that this delay is directly injurious. As far as our present knowledge goes, anæsthesia is best accomplished between the limits of seven and ten minutes, so that the percentage to be taken should be such as will lead to this result.

It was found on actual trial that if the percentage was made to have the same value as the time (up to two minutes) the result was very satisfactory. In other words, at the half-minute .5 per cent. was administered, at one minute 1 per cent., at a minute and a half 1.5 per cent., and so on. After two minutes the rise was made more gradually; in an average case 2½ per cent. was reached in three minutes, and, if necessary, 3 per cent. in four minutes.

No fault could be found with the result of this routine. It is very comfortable for the patient, anæsthesia is very gradual, and there is a marked lessening of the so-called stage of excitement often seen with the open method. But the words "if necessary" indicate another difficulty, which from this point onwards has always to be borne in mind, and that is the state of the patient. It is not possible to blindly administer any percentage without the most careful observation of the result, and, although we can now go so far as to say that in an average case such and such percentage will probably do such and such things, yet the anæsthetist must always be on his guard for the individual variations of each patient which play such an important part in the human subject.

The reader will now be in a position to understand how a "chloroform curve" is constructed. Time in minutes is plotted along the abscissa, percentages of chloroform on the ordinates, and from such a diagram it is possible to see at

a glance the whole history of any one case with the various incidents graphically represented (Alcock, 7 *b*, *c*).

Fig. 1 may be taken as a typical example. The gradual rise

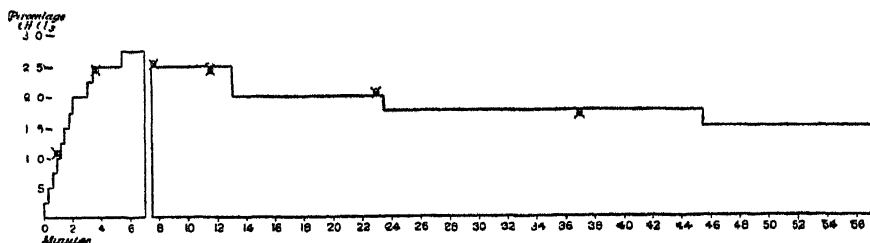


FIG. 1.—Typical case. Description in text.

up to 2.75 per cent. is first to be noted. The patient was a boy aged nine, so that surgical anæsthesia was easily reached in seven minutes. At this point there was a little break in the curve, which marks the time at which the anæsthetist considered that the patient was ready to be taken to the operating-theatre. During the move no chloroform was given, and the administration began again at 2.5 per cent., and the subsequent history of the case is written in a declining series of steps—2 per cent., 1.75 per cent., to 1.5 per cent.—and at the fifty-seventh minute the operation was finished and the anæsthetic stopped.

In one respect this curve differs from the rest. At six points there are to be seen little crosses, the centres of which indicate actually measured percentages. This was the final test of the apparatus. It was carried out in the following manner. After the operation the curve was plotted out in the usual way. A week later the machine was taken to the laboratory, and the operation gone over again in dummy, a densimeter bulb being placed at the end of the outlet tube instead of the patient. At the points marked, estimations were made, and it can be seen how closely, even on a different day and in a different place, the nominal percentages agree with those actually measured.

FOUR-FIFTHS OF MANKIND

In considering the curves from a large number of cases the most important result is apt to be forgotten. When, as in the present series, forty of the cases present no very obvious feature, while ten show points of difference, one is apt to consider the latter interesting and the former dull,

merely because the same phenomena are repeated time after time. And yet it is plain that the curves from these forty average cases deserve quite as careful study as the ten exceptional cases, if only from their greater number. All these



FIG. 2—Typical case.

curves show the same gradual rise, the break, and the gradual fall, the latter varying from time to time according to the necessities of the operation. Accordingly, the comparison of the curves after the break is not easy; but in the stage of induction the same objections do not apply, so that this gap forms a convenient and not very inaccurate landmark.

It might be supposed that it would be possible to express numerically from the curves the dose of chloroform that each patient has had to produce anaesthesia, taking the break as the end point. But the data are in reality insufficient—the dose that each person actually gets is not measured by the percentage in the inspired air, but by the *difference* between this percentage and that present in the expired air. This latter has not been measured in the present series of observations, and as it is plain from the experiments of Vernon-Harcourt (4*d*) and Brodie and Widdows (*l.c.*) that this varies from time to time, it is not possible to make a general correction. In addition, there is some evidence to show that, even if two patients retained the same quantity of chloroform, it would not necessarily produce the same result, so that this problem must be left to a future occasion. All that can be said at present is that the forty average cases required about the same percentage from the machine, and of the ten exceptional cases some required more and some less.¹

¹ Dr. Levy has published (5*e*) a very interesting series of cases anaesthetised with exactly 2 per cent. of vapour. The time taken to induce anaesthesia varied between six minutes and over twenty minutes, showing clearly the amount of

It is possible, however, to make an approximate comparison between cases anæsthetised by the open method and the present series. It was found that the time of induction by both methods was about the same. It follows that approximately the same percentage must have been given, but the individual variations were very much greater with the open method, and depended more on the anæsthetist. The results of the skilled professional were very like the curves just given; while in those of the comparative beginner one would judge that the percentage at the beginning was greater than the optimum, and, later on, rather less.

One curious result sometimes occurs with the open method. Although at least 4 per cent. can be obtained from a cone of lint (see fig. 8 later), sometimes the anæsthetist complains that, do what he will, the patient will not remain completely relaxed. It is customary to lay the blame on the particular brand of chloroform employed, but, as every brand is accused in turn, and as this difficulty is not found with the machine, it is probable that some other explanation must be sought.

In some of the forty cases the operation was prolonged for a considerable time. It is interesting to note how that, for a very long period, the patient can be kept at a perfectly even degree of anæsthesia with the same percentage of chloroform. This evenness appears to depend on two factors, the first being the steady percentage of chloroform, and the second the nature of the operation. If the first factor is kept constant it is possible to study the second, and a very curious observation was made on a point which is of some importance. A patient is at the even stage of anæsthesia just described, and the corneal reflex is just absent. Now, if any violent afferent impulse is set up (if, for instance, the parietal peritoneum is pulled to allow of the sutures in it being tied), the patient becomes less deeply anæsthetised, the respiration increases in depth and frequency, the blood-pressure falls, and the corneal reflex comes back. The analogy is very close to the condition seen in morphia poisoning, where the subject can be aroused by shaking or other measures, to fall back again into unconsciousness when

individual variation. From the considerations mentioned above it follows that it is not possible, even in these cases, to say that each patient retained the same amount when breathing in 2 per cent. vapour, and so the dose cannot be calculated exactly.

the stimulus is removed. It would seem that one effect of both these drugs is to produce a condition very closely to sleep, from which the patient can be aroused by appropriate excitation.

THE EXCEPTIONS

While the study of the exceptional cases raises many interesting and still unsolved problems, it can hardly be said that the present series of observations does more than call attention to what remains to be done. If one simply takes the cases as they come, it is plain that one must by the law of averages deal with a very large number before the exceptional occurrences come sufficiently often to permit exact conclusions to be drawn; so that the remarks in this section are only put forward as imperfect notes of what seems probable at the present time, and it is quite possible that further work may materially alter some of the conclusions now arrived at. However, bearing this caution in mind, it is legitimate to consider certain of these exceptional cases, of which the explanation is not entirely without reasonable basis.

It was found that, on the whole, children required less chloroform than adults, both for the induction and for the maintenance of anæsthesia (fig. 3); but whether this is due to



FIG. 3.—The Chloroform Curve in a Boy aged 11.

their age or merely to their lesser weight could not be accurately determined. In the case of animals, this point can be ascertained with much greater accuracy; and Embley (*loc.*) found that old dogs took much longer to become anæsthetised with the same percentage than young dogs of the same weight.

Thin people also took a smaller percentage than the average, even when of adult age (fig. 4); but occasionally a puzzling case is met with, as occurred in one of Dr. Levy's patients, where, for no reason that could be detected, a thin woman required

an altogether unusual amount of chloroform. On the other hand, one case occurred in the present series, where the patient had a tendency to goitre, and where much less chloroform was required than the normal.

Distinctly fat people, as a rule, require more than the usual amount. It happened that on one day two patients presented themselves for the same operation, by the same surgeon—one a thin man, the other a lady of very comfortable dimensions; and the two curves, reproduced as figs. 4 and 5, give an interesting contrast.



FIG. 4.—Thin Patient taking little Chloroform.

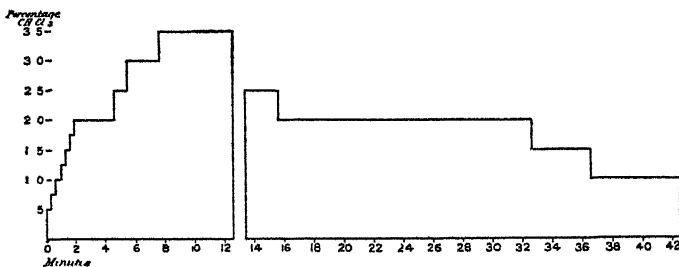


FIG. 5.—Stout Patient taking much Chloroform.

It is not yet certain that the different amount of adipose tissue is the important feature in producing this result. Dr. Levy's negative instance would point to some other cause, still unknown, that might have to be considered. Fat persons often have a comparatively inefficient lung-ventilation, and this certainly would diminish the amount of chloroform absorbed, and so render a higher percentage necessary. But the idea is not at all unreasonable that the adipose tissue subtracts chloroform from the blood and stores it up in a comparatively inert place; and so the cells of the cerebral cortex would get less chloroform than would otherwise be the case.

Persons who habitually take alcohol to excess require more chloroform than the normal amount (fig. 6): how much more

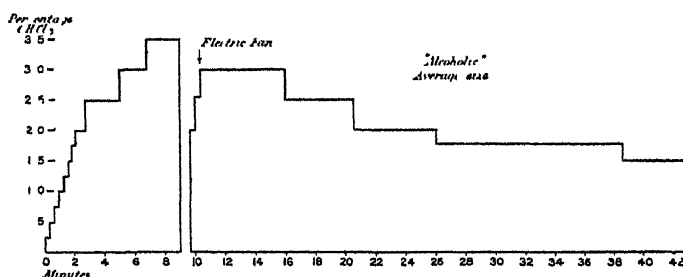


FIG. 6.—Alcoholic Patient taking more than the average.

cannot be definitely stated. Occasionally one finds an individual who combines several of these characteristics. One such is represented by the curve (fig. 7). The patient, a big, thick-set, muscular man, required immediate operation as a result of an "accident," which took the shape of stab just below the liver, received in a drunken brawl. After nearly half an hour, during which he inspired 3 to 3.5 per cent. chloroform vapour, the anæsthetist had to resort to the open method to keep the abdominal muscles sufficiently relaxed.

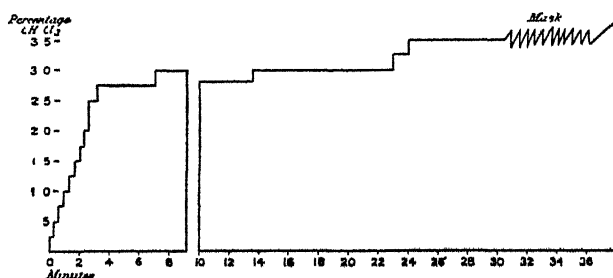


FIG. 7.—Thick-set, muscular, alcoholic Adult taking much more than the average amount. Note that the "mask" gave at least more than 3.5 per cent.

An altogether different class of case is sometimes found, where there is good reason to suspect that the dangers attending the use of any general anæsthetic are much greater than usual. One of these persons, an elderly, corpulent woman, was known to have a marked degeneration of the heart muscle. Ether would have been dangerous from the tendency to cause bronchitis that often occurs with this

anæsthetic, and chloroform is certainly not a heart tonic, whatever other properties it may possess. Advantage was, therefore, taken of the stimulating action of oxygen, which Leonard Hill has shown to have so marked an effect when the heart is subjected to an unusual strain. Oxygen was blown through the apparatus instead of air at the beginning of anæsthesia, and again towards the end of the operation, and the patient left the operating-table in a better state as regards her heart than when the operation began, and recovered without a bad symptom. This is in full accord with the observations of W. Hagen (13), who has published records (made during the use of the Roth-Dräger apparatus) of the blood-pressure and respiration under oxygen and chloroform.

One case was exceptional from a different reason to the others. The anæsthetist was using the apparatus for the first time, and found some difficulty in translating his knowledge (admittedly great) of the open method into the percentages given by the machine. So that the known percentages in this case were not the correct ones, and, after various unexpected incidents had occurred (fortunately none of them of a serious character), the anæsthetist finished the case with the open

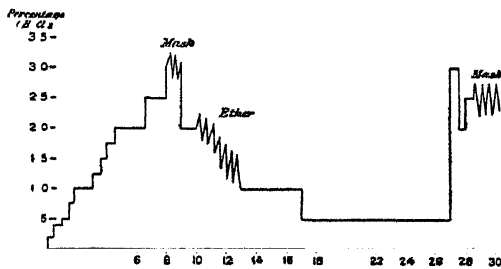


FIG. 8.—Known percentages that were incorrect.

method (fig. 8). But it is only fair to say that on all the subsequent occasions the results were all that could be desired.

CONCLUSIONS

It is a little misleading to write of the conclusions to be derived from observations like the present, which are in reality but the commencement of further researches of which the end is still far off. But it is sometimes useful to look back, even if

only for a short distance, and take a survey of the field that has been traversed, as a guide to the work that remains to be done.

The first point that has been established is that anæsthesia with known percentages of chloroform is more easily accomplished, more pleasant to the patient, and possesses fewer elements of danger, than by the open method of unknown percentages. When this latter method is carried out by experts the difference is not great, but every one (even every medical practitioner) who may be called up to give anæsthetics is not necessarily thereby made an expert, and the results may (some I have seen do) fall considerably short of the ideal.

It is necessary for the success of the method with known percentages that these shall be correct percentages. What these are must be determined by direct observation. From the cases already anæsthetised we can say that in the great majority a percentage gradually rising to 2·5 per cent. and probably 3 per cent. is required, if anæsthesia is to be obtained in from seven to ten minutes. Later on, the patient requires less chloroform—2 per cent., 1·5 per cent., dropping to 1 per cent. or even less if the operation extends much beyond an hour.

All this seems so simple that the advantages of the method are not at first realised. The chief of these is that the anæsthetist has now another guide in his procedure besides the condition of the patient. Hitherto this latter factor has been the only thing that was known. If the dose was correct the patient was anæsthetised, if it was too little he came round, if too much he died. As in most patients the same amount of chloroform produces the same symptoms, the open method works better in practice than might be supposed; but, when an exceptional case is met with, the anæsthetist can do little more than guess at the amount of the drug that has produced any given condition. An excellent instance of the uncertainty attending this point can be found in the controversies that periodically arise as to the cause of death in chloroform anæsthesia, one school holding that too much has been given, the rival school believing that (*mirabile dictu*!) the patient has had too little!

When the percentage in the inspired air is known, at least one cause of uncertainty is removed. Even allowing for every possibility of individual variations, a patient that is being kept

under at 1 per cent. is not likely to get an overdose (at least, in the first hour of anæsthesia); while, if it is necessary to go up to 3 per cent. for induction, the anæsthetist knows that this is a percentage that may easily become dangerous, and is proportionately careful. There is, therefore, no place left for "accidental" deaths by this method.

But even with this great advantage it must not be thought that the anæsthetist can ever relax his vigilance. A percentage that is "safe" at one time may very easily cease to be so at a later stage, or in a different patient. The study of individual variations will serve to put the anæsthetist on his guard and give him some idea of what percentage to use, but can at present do little more.

One thing is, however, very clear. Any haste in the induction of anæsthesia is fraught with especial danger by any method. It is only safe to go as high as 3 per cent. if this is done very gradually, and this gives anæsthesia in from seven to ten minutes. What, then, can be said about the induction of anæsthesia in five minutes? Obviously a higher percentage must be used, and used rapidly; and if the patient, under these conditions, escapes with his life, it is more than the anæsthetist has any right to expect. Embley's motto, "Take time to put the patient under," should be painted in letters of gold on the walls of each anæsthetising-room. In short, if there is any reason for haste, chloroform should not be used at all.

In bringing this paper to a conclusion, I may be perhaps allowed to add a personal note. A research of this kind can only be brought to a successful issue by the kind co-operation of others, and I feel that I owe to the help and advice of my colleagues at St. Mary's Hospital more than can be expressed in the usual formal acknowledgment of thanks.

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RECENT RESEARCH ON INDIGO ¹

By H. H. ROBINSON, M.A., F.I.C., F.C.S.

INDIGO is a blue dyestuff owing its tinctorial properties chiefly to a chemical substance known as "indigotin"; of this Bengal indigo contains about 60 per cent., and of the other ingredients some are considered to have a beneficial effect when indigo is used as a dye. Indigo is obtained from a plant (*Indigofera tinctoria*, Linn.), which however does not contain indigotin but a colourless glucoside "indican," which on hydrolysis yields dextrose and indoxyl; the latter by oxidation becomes converted into indigotin. In the process of manufacture the leaves are steeped in water and the liquid obtained is run off and submitted to the oxidising action of the air, whereby indigotin is formed as an insoluble blue precipitate; this is removed, dried and sold as indigo.

Indigo has long been known, mention of it occurring in the works of Pliny and Dioscorides, who both wrote about 100 A.D.; the names they used, "Indicum" and "Ἰνδικόν," imply that it was of Indian origin. The importation of indigo in large quantities into Europe from the East began about the commencement of the seventeenth century, and it became a serious rival to woad, a blue dyestuff prepared from the woad plant (*Isatis tinctoria*, Linn.), which also contains a substance yielding indigotin. The use of indigo provoked the opposition of powerful interests in Europe that were concerned in the production of woad, but after a struggle it succeeded in displacing the use of the latter; in the closing years of the nineteenth century, however, it has been threatened in its turn by artificial indigotin produced from coal-tar products in the laboratories of Germany.

¹ *Report to the Government of India, containing an account of the Research Work on Indigo performed in the University of Leeds, 1905—1907.* By W. Popplewell Bloxam, B.Sc. (Lond.), F.C.S., F.I.C. With the assistance of S. H. Wood, B.Sc. (Lond.); I. Q. Orchardson, B.Sc. (Aberdeen); R. Gaunt, Ph.D. (Berlin), M.Sc. (Leeds); and F. Thomas, B.Sc. (Manc.), F.C.S.; and under the general supervision of Mr. A. G. Perkin, F.R.S., F.R.S.E., F.I.C., of the University of Leeds. [Pp. 117.] (Published by Order of His Majesty's Secretary of State for India in Council, 1908.)

One task which occupied the attention of chemists during the last century was the "synthesis" of various well-known carbon compounds—that is to say, the discovery of processes whereby these compounds can be built up from their elements in the laboratory without the aid of the processes that go on in plant and animal life. Step by step, as the result of patient researches, the synthesis of many plant and animal products has been effected; in the case of indigo an important discovery made by von Baeyer and Emmerling of a method of obtaining indigotin from isatin was published in 1870. At that time isatin had not yet been synthesised, but eight years later a method of preparing isatin artificially was published by von Baeyer and thus the synthesis of indigotin was complete. In 1880 von Baeyer published another and more convenient way of attaining the synthesis, and continual additions have now been made to the possible ways of synthesising indigotin; benzene, toluene, and naphthalene being the different starting-points. The methods of obtaining it from naphthalene, a very cheap substance, obtained in the manufacture of coal gas, are those that most threaten the natural indigo; and in July 1897 artificial indigo, most probably made from this source, was introduced into the market by the Badische Anilin und Soda Fabrik.

Indigo has been manufactured from time immemorial in India, and in the last century its production formed a most important industry carried on by numbers of European planters and giving employment to numerous natives; in 1895-6 the exports by sea amounted to 9,350 tons, valued at £3,570,000, and in 1901 the number of persons employed was given as 173,000. The discoveries of 1880 gave rise to some alarm among the planters, but it was not until the last years of the century that they called in the aid of scientific assistance. With the help of the Government they have been endeavouring to make up for lost time and avail themselves of the methods of modern science.

In order to improve the indigo industry in respect to both the growing of the plant and the manufacture of indigo from it, the point of primary importance was the question of analytical control. Without analytical methods, capable of accurately determining the amount of indican in the plant and of indigotin in the indigo products, the results of field experiments to improve the yield of indican and of attempts to ensure the maximum yield of indigotin in the course of manufacture could

never be known with certainty, and consequently afforded unsatisfactory guidance as to the best courses to adopt and the direction which improvements should take. It was to the working out of such analytical methods that Mr. Bloxam and his assistants devoted their attention, with the results described in the present Report.

The problem of finding out the percentage of a plant product in a mixture is not easy, and in the case of indigotin is made harder by the fact that this substance is only sparingly soluble in liquids other than those of high boiling-point; thus its separation from other substances is not easily accomplished, as these go more or less into solution in the solvent used. Mr. Bloxam's solution of the problem is to heat the crude indigo with fuming sulphuric acid containing a certain proportion of sulphuric anhydride; this converts the indigotin into indigotin-tetrasulphonic acid, which forms a blue solution when the liquid is diluted with water. This acid, it was found, has the property of forming a potassium salt which is practically insoluble in cold strong potassium acetate solution; accordingly on mixing the blue solution of the acid with strong solution of potassium acetate the potassium salt is formed by double decomposition, and when formed is precipitated owing to its insolubility in the excess of potassium acetate solution. The precipitate can be got into solution by the aid of heat, and on cooling with ice separates again in a crystalline form which can be easily filtered off and washed with cooled potassium acetate solution; the filtrate and washings contain only a trace of indigotin. The precipitate on the filter is then dissolved in hot water, and the amount of indigo in it is determined volumetrically by using potassium permanganate or titanium trichloride. The solutions are standardised by means of indigotin obtained in a pure state by sublimation; and precautions to be taken in performing this last operation are given by the authors. The merit of the method lies in the fact that the precipitation and washing of the indigotin compound afford an opportunity of getting rid of impurities which would otherwise render the volumetric results incorrect.

In order to make sure of the effectiveness of the method in separating impurities the authors used preparations of various impurities occurring in indigo, namely indigo-gluten, indigo-brown, and the indigo-yellow or kaempferol, which latter substance occurs in the indigo from the Natal or Java indigo

plant (*Indigofera arrecta*), and investigated their effect when mixed with pure indigotin and analysed by the method. They found that the indigotin was correctly determined in presence of the first two impurities, but that a special treatment had to be adopted when kaempferol was present.

As regards the other problem, the determination of the amount of indican in the leaf, the plan that first suggests itself is that of converting the indican into dextrose and indigotin by hydrolysis and oxidation, and then determining the amount of the latter substance; it has however been found that the full amount of indigotin is not obtained when this is attempted, owing no doubt to the formation of a certain amount of other substances, and thus a determination on these lines gives too low a result. Advantage has been taken by the authors of a suggestion of Beijerinck's, namely, to use the property possessed by indoxyl of combining with isatin to form indirubin; the solution of indican is boiled with isatin and hydrochloric acid, the acid effects hydrolysis into dextrose and indoxyl, and the latter combines with the isatin to form indirubin, which is filtered off, purified, dried and weighed; air must be excluded during the operation by means of a current of carbon dioxide. The authors found the method satisfactory, and verified its accuracy by experiments with pure indican.

Now that it is possible to examine the leaves and the products in the various stages of manufacture with much greater accuracy than was previously the case, there is hope that great improvements can be made both in growing the plant and obtaining from it a much better yield of indigotin. To give an instance of this: the action that goes on in the steeping process seems to be the conversion of the indican into dextrose and indoxyl by the action of an enzyme present in the leaves; in some experiments of the authors this enzyme was used to decompose indican, and indigotin was then obtained by oxidising with air, thus imitating the present practice, and it was found that at the best only 68 per cent. of the full yield of indigotin was obtained. This and other evidence point to the present process of manufacture being one of no great efficiency; and it is to be hoped that the Government will aid the continuance of the present researches and give the indigo-growing industry every chance of holding its ground against the competition of the artificial product.

THE PHYSICAL CONDITIONS AT THE BEGINNING OF LIFE

By CARL SNYDER

WHEN we endeavour to conceive the conditions obtaining at the earliest appearance of life on the earth, or at least life as we know it, we are forced to the conclusion that those conditions were probably not immeasurably different from such as obtain now.¹ This is the unmistakable trend of present-day investigation, and is true whether we still cling to the classic idea of an igneous earth cooling down, as in the nebular theory, or give adherence to the idea of a little earth growing up, as in the meteoritic hypothesis as remodelled by Chamberlin and Moulton.

The temperature may have been somewhat higher, but it is fairly certain that it could not have been much above 75° C., and probably was considerably less. Whatever else life may be, physically it is a reaction of colloids in solution, and these colloids begin to coagulate at even moderately high temperatures (60° C.).

There may have been no continental land²; but, as we shall see, unless we accept the cosmozoic theory of germs brought from elsewhere in space,³ terrestrial life could scarcely have begun in the sea. Unquestionably, however, the chief stages in the evolution of living beings were passed in the sea-water, and there is reason to believe that the salt constituents of the ocean then were somewhat different from now. For example, it seems possible that the early ocean was relatively limeless.⁴ But in the light of our new knowledge as to the probable chlorination of the sea by volcanic action,⁵ there seems little

¹ A. C. Lane, "Early Surroundings of Life," *Science*, 26, 129, 1907.

² Ratzel (*Raum und Zeit*, 1908) doubted if there was ever a landless earth.

³ Cf. Arrhenius, *Werden d. Welten*, p. 191.

⁴ Macallum, "Paleochemistry of the Ocean," *Univ. of Toronto Studies*, No. 5, 1904; R. A. Daly, "The Limeless Ocean of pre-Cambrian Time," *Am. Journ. of Science*, 23, 93, 1907.

⁵ Brun, *Arch. d. Sciences de Genève*, 19, 439, 1905; 22, 425, 1906.

evidence of a fresh-water ocean such as some geologists have imagined.¹

If a considerably higher temperature prevailed, the atmosphere would have contained much more water vapour than at present, and there would have been a much greater cloud formation.² A variety of evidence goes to show that the primeval atmosphere was devoid of free oxygen,³ and it may have contained much more hydrogen than the present. But in the light of recent physical investigations it seems certain that the old idea of an enormously greater quantity of carbon dioxide has no probability in fact.⁴ Any additions of carbon dioxide would have been largely absorbed by the ocean, and beyond a certain point this dissolved carbon dioxide would have been precipitated in the form of carbonates.

If we adhere to the old idea of igneous origins, the vast quantities of carbon contained in the rocks would have been combined as carbides, metallic or silicious, as the researches of Moissan,⁵ Gautier, and Brun have pretty clearly demonstrated; and carbon dioxide would have been evolved gradually through volcanic action, as at the present time. On the other hand, if we conceive of a slowly growing earth, the atmosphere, including carbon dioxide, was slowly evolved through the extrusion of the gases occluded in the falling meteors. Originally the quantity of atmosphere would necessarily have been small, and it would have attained its present proportion only through a slow growth.⁶

Again, in the light of present physical theories, we must conceive that if the earth condensed from a gaseous mass, the time elapsing between the first formation of a hot crust, and the cooling of this to something like the present temperature, would not have been long.⁷ A solid crust would have formed at somewhere not very far from 1000° C., and, according to Kelvin and Arrhenius, the time required to cool this mass to below

¹ Cf. Joly, *Proc. R. S. Dub.* 7, 23, 1899.

² Cf. Manson, *Climat des Temps géologiques*, 1906.

³ Stevenson, *Phil. Mag.* 50, 312, 1900; Kelvin, *ibid.* 47, 66, 1899.

⁴ Tolman, *Journ. of Geol.* 7, 585, 1899.

⁵ *Proc. Roy. Soc.* 60, 166, 1896; Brun, *l.c.*; Gautier, *Ann. d. Mines*, 9, 316, 1906.

⁶ Cf. Chamberlin, *Journ. of Geol.*, vols. 5, 6, 7; McKee, *Science*, 23, 271, 1906; Clarke, *Data of Geochemistry*, 49, 1908.

⁷ Arrhenius, *l.c.* p. 36; Kelvin, *l.c.*

100° C., the boiling-point of water, was probably but a few thousand years ; and the drop from this to somewhere near the present temperature possibly required only a few hundred years. This would have been a period of great chemical change ; and although it is perfectly conceivable that in this period reactions may have taken place resulting in the basic compounds of living substance, our knowledge of chemical processes at high temperature is as yet too slight to permit of any practical deductions or theories.¹ But if anything is certain, it is that the quantity of carbon dioxide was never large, and, as we shall see, the origin of life from a carbon dioxide or carbonate basis is the simplest conceivable theory.

On the meteoritic or planetesimal hypothesis, the only conceivable conditions wherein a sufficient concentration could occur of the substances for the organic or vital synthesis would be in volcanic action.

In other words, it seems probable, in the light of our present knowledge, that our ideas as to the terrestrial Genesis of Life will follow much the same course as that of all physical theories, and notably those of geology. The beginnings will be the most fantastic imaginings—as, for example, Kepler's supposition that the earth is a vast breathing animal. These fancies of intellectual childhood will in time be replaced by more philosophical conceptions, reaching a period akin to the catastrophic stage. This is represented by pictures of a boiling, sizzling earth, with a continuous downpour of hot hydrochloric and other acids, an atmosphere saturated with carbon dioxide and under tremendous pressure, several hundred times that of the present, as in the ideas of Sterry Hunt² and others. And finally we shall come, as in all our scientific theories, to perceive that change is slow, that the observable forces of the present day are adequate for the explanation of events in the past, that more or less what is, was. In brief, we should probably find, could we recall it, that the primeval earth would not be so astonishingly different from the earth as we know it now.

While our ideas on the physical side have been, and are still, undergoing this process of simplification, a like process has been at work in the field of pure biology. We are now fairly agreed that the myriad forms of life are due to a slow

¹ Clarke, *l.c.* p. 232.

² *Chem. and Geol. Essays*, p. 35, 1874.

process of evolution—that, for example, giraffes, camels, hippopotami, and the monsters of the mesozoic swamps were not each the result of a separate act of creation. There are now few to doubt that our distant ancestors were extremely simple forms, like the fungi or moulds, the algæ that form the scum of ponds, the bacteria whose invasions we have learned to dread. We know that these and all other living forms are, chemically, a fairly definable substance which we call protoplasm; that the chief constituent of this protoplasm is a complex compound which we call protein; and we are coming now to understand pretty well the nature of protein.¹

As recent work in the physiology of digestion² and in the germination of plants, and the syntheses of Emil Fischer³ have shown, the proteins in turn are made up of compounds known as amino acids; these are, in Fischer's phrase, the basic substances or "Bausteine" of all living matter whatsoever. These amino acids themselves represent varying compounds of a carbohydrate, a simple sugar or kindred combination of carbon and water, with the radicle of ammonia, NH_2 . Very recently Ciamician⁴ has shown that an amino acid may be derived from the reaction of prussic acid on acetone, under the influence of light. In turn, W. Löb has shown that formaldehyde may be produced by the action of the silent electrical discharge on a mixture of carbon dioxide and water.⁵

It is true that no one has as yet been able to produce life from amino acids, any more, for example, than a few years ago any one was able to devise a machine and fly through the air like a bird. There was no very great mystery about aviation, though the practical difficulties seemed insuperable. In the same way, to those who have followed the splendid development of bio-chemistry within the last five or ten years, and especially within the last one or two years, there is no very great mystery about the chemical nature of life.⁶

The distance passed over in the complication of a protozoan to the marvellous mechanism which is man, or from a proto-

¹ Mann, *Chem. of the Proteids*, p. 8, 1908.

² Starling, *Recent Advances*, p. 8, 1906.

³ *Untersuchungen*, 1906.

⁴ *La Chimica organica negli Organismi*, p. 28, 1908.

⁵ *Zeit. f. elek. Chemie*, 12, 282, 1906; *Landw. Jahrb.* 35, 54, 1906.

⁶ Cf. Loeb, *Dynamics of Living Matter*, p. 7, 1906; O. Loew, *Chem. Energie d. leb. Zellen*, p. 6, 1906.

phyte to a giant redwood, is immeasurably, almost infinitely greater than the step from a layer of colloidal polyfetides, in a salt solution, to the mucilaginous smear which we call Myxomycetes. There is infinitely greater mystery in the evolution of a sentient being, a Newton, a Wagner, from an amoeba or its prototype, than in the process by which the phosphorised protein or granular substance of the cell-nucleus becomes capable of auto-synthesis—that is to say, in the only decisive sense of the word, alive. It is infinitely more of a wonder how an almost formless microscopic germ-cell becomes a thinking mechanism, which may weigh the sun or hear in its brain the unwritten, unsounded strains of the *Symphonie Pathétique*. This marvellous unfolding we may trace, literally, step by step from the first undifferentiated layers of the embryo to the being quivering with a myriad impressions and influences, of which the music is the unconscious echo.

We have a fairly definite measure of these biological "distances," in the simple question of size. An adult man is on the average at least twenty million times in weight the microscopic germ from which he springs.¹ The giant fin-back whale is hundreds of millions of millions of times the size of the smallest bacterium known. On the other hand, it is now known that there are living beings containing at most not more than a few hundred colloid particles,² and it seems extremely doubtful if these individual particles are alive (*i.e.* self-reproducing).

To say, therefore, that there is more mystery in a bacterium or a germ-cell than in the individual development of a human being or in the historical evolution of life on earth, is utterly to misrepresent the present state of our knowledge. We can describe the successive stages by which the germ becomes a man; we can imaginatively reconstruct the process by which a bacterium or some other unicellular form became in the course of a hundred million years a hundred-ton whale. But we have no clear idea as to the causes of differentiation nor the factors of evolution.

It is equally true that we have as yet no clear idea as to how in nature a simple amino acid becomes a complex phos-

¹ Cf. Muhlmann, *Ursache des Alters*, 1908, for lit.

² L. Errara, *Rec. de l'Inst. Bot. Brux.* 6, 73, 1903; MacKendrick, *Rep. Brit. Asso.* 1901, p. 808.

phorised protein or nuclein, nor how this substance acquires the capacity of auto-synthesis or reproduction, which makes it alive. But it is now fairly clear that it is through a series of complications and chemical differentiations akin to that by which, for example, the endless varieties of precious stones, sapphires, rubies, emeralds, topaz, chrysoprase, have been synthesised in the earth beneath our feet.

Probably it will be a very long time before we shall be able to take a simple organism, a fungus or a protozoan, and turn this at will into a star-fish or an oyster. Doubtless the generality of biologists believe that this will never be possible. Still less may we hope, perhaps, to convert a protozoan into a fish, a bird, or a man. But it is not at all inconceivable that we shall some day, and perhaps not distantly, mix in a test tube a substance which will indefinitely reproduce itself from non-living materials, and this will be the creation of life.

Herein, perhaps, we have a picture, and a measure, of the amount of mystery involved in the familiar and accepted fact of evolution, as contrasted with that of the organisation of non-living into living matter. And if in this we do not err, it is evident that the natural origin of life on the earth is not an insoluble riddle, and that even now it is possible, in a broad way, to realise the conditions under which this evolution from the inorganic took place.

Clearly to state a problem, it is often remarked, is half its solution. It has not been possible, until the most recent achievements of bio-chemistry, to state the problem of the initial appearance of life with any clearness at all. Until very recently it was not known what "protoplasm" is. Until very recently it was not known how inorganic substance might be built up into compounds closely resembling the basic substances of protoplasm. It was for this reason that any theory as to the origin of life was, until the year 1906, for all practical purposes nothing but a confused guess.

In the light of our new knowledge, we may state the problem in the following precise form :

1. Under what conditions did carbon dioxide or some other carbon compound unite with water to form formaldehyde or other simple carbohydrate, and how did this carbohydrate react with a nitrogen-containing substance like ammonia or prussic acid, to form amino acids?

2. How did these amino acids polymerise, or unite among themselves, to the formation of complex polypeptides or proteins, and how did these in turn unite with phosphoric acid to form nucleo-proteins?

3. In what way, and in what sort of a medium, did these nuclein compounds acquire the capacity of auto-synthesis, and under what conditions would the products thus automatically formed unite in loose colloidal union to ultra-microscopic granules and then microzymes, chromosomes, spheroplasts, and their like?

4. Under what physico-chemical conditions were these living granules associated to form the first simple cells?

Assuming, as we are justified by a multitude of inferences from its present-day reactions, that living substance was not in principle different at its beginning from what it is now, we may consider what conditions are implied from this conception of the problem.

Formaldehyde, together with a variety of similar substances, as for example formic acid, has recently been synthesised by W. Löb¹ by exposing a mixture of carbon dioxide and water vapour to the action of the silent electric discharge. This reaction is endothermic, that is, accomplished with the absorption and not the liberation of energy. The conditions of this synthesis are, first, a sufficient concentration of carbon dioxide and water vapour; second, an adequate energy supply for the dissociation of the carbon dioxide and water molecules. It has also been shown by Horstman Fenton² that a similar synthesis of formaldehyde may be obtained from an aqueous solution of carbon dioxide and water, in the presence of magnesium—this reaction being enhanced by the presence of weak bases. It is to be noted that the synthesis of formaldehyde or its polymeres in the plant is accomplished in the presence of magnesium (chlorophyll) under the action of light, and it is known, from a long series of researches, that the synthetic action of light and the silent electric discharge are practically identical.

Driven to a choice between these two possibilities, we may note that a sufficient concentration of carbon dioxide or of magnesium in the primeval ocean seems, so far as we may

¹ *Loc. cit.*

² *Chem. Soc. London*, 91, 687, 1907; *Chem. News*, 1907.

judge, improbable. On the other hand, a sufficient concentration of carbon dioxide and water vapour in the primeval atmosphere seems equally improbable. We are thus forced to the conclusion that so far as we know the initial organic synthesis took place neither in sea-water nor in the ordinary air.

We reach the same conclusion when we study the further reactions of the primitive carbohydrates with a nitrogen compound, such as ammonia or prussic acid. Small quantities of ammonia have been detected in the atmosphere, especially following a violent electric storm, and likewise kindred nitrogen compounds, such as nitric acid. But the quantity of these is excessively slight; and what is true of the atmosphere is equally true of sea-water. So far as our knowledge extends, therefore, the formation of amino acids seems impossible under any conditions which we may imagine in the atmosphere or the ocean, past or present.

Through consumption by plants, by rock-weathering and the like, enormous quantities of carbon dioxide are annually drawn from the atmosphere, and yet neither historically nor, so far as we may judge, geologically, is there any evidence of the exhaustion of the supply. Apart from the amount returned to the atmosphere by the exhalations of animals and the decay of vegetable matter, practically the sole sources of the atmospheric supply of carbon dioxide are in volcanic action and in gas springs. The quantity given out by the latter is not large in the aggregate, nor are the conditions of organic synthesis to be found in the springs. We are thus led to consider whether such conditions are present in volcanic action.

Within three or four years our ideas of volcanic chemism have been very radically enlarged, chiefly through the work of Armand Gautier, following up the pioneer work of Moissan, and by the researches of A. Brun.¹ For very obvious reasons our knowledge is as yet tentative, and it is not surprising to find these two able investigators somewhat at variance in their conclusions. But it seems now clear that the rôle of volcanic action in earthly affairs is vastly more vital and dominating than had previously been supposed. It is not only the chief source of carbon dioxide, and therefore possibly the regulator of terrestrial temperature, but likewise the chief producer of ammonia or nitrogen compounds (cf. Brun's re-

¹ *Loc. cit.*

searches), which is a second condition of the continuance of terrestrial life; is the chief source of the chlorination of the sea,¹ and also probably the chief source of phosphoric acid.

It was long assumed, from the enormous cloud-formation and the heavy downpour of rain that usually follows, that the chief eruptive constituent was steam. This, however, has lately been called into question by Brun, who has shown that in some eruptions water vapour may be completely absent. It is, however, well established that in an eruption there are often quantities of hydrogen. This, with the other eruptive gases, carbon dioxide, ammonia, hydrochlorides, etc., would be driven into the air with explosive force, together with the usual scorïæ and volcanic dust. On this dust the water vapour of the atmosphere condenses, while the hydrogen burns, uniting with the atmospheric oxygen to form more water, so that, whether steam is part of the eruption or not, heavy rainfall is a familiar if not invariable characteristic.

We should have, then, in the upper levels of an eruption, a concentrated mixture of carbon dioxide, water vapour, deoxidised air, free hydrogen, and various hydrogen, chlorine and sulphur compounds, with, in all probability, large quantities of ammonia. This latter, uniting with the hydrochloric acid, gives rise to sal-ammoniac, which is a familiar volcanic product.

Yet another characteristic of an eruption is an incessant play of lightning. This may be due to the condensation of the atmospheric water vapour, as in the ordinary formation of thunder-clouds, or to gaseous friction from the explosive force with which the gases are driven into the air, as in an Armstrong hydro-electric engine.² In any event, the electrical potential is there, and in the presence of water vapour it is probable that it would be manifested in the form of the silent discharge, as well as in the more violent bolts of lightning.

In an eruption, therefore, conditions apparently exist practically identical with those under which Berthelot, Löb and others have realised their syntheses. The volcanic formation of formaldehyde and its congeners seems readily conceivable. We know of ammonia in abundance, and the formation of prussic acid, especially in a primeval atmosphere devoid of free oxygen, seems more than probable. One or the other of these

¹ Cf. Suess, *Geog. Journ.* 20, 520, 1902.

² Judd, *Volcanoes*, p. 28, 1881.

substances, or both, are present to unite with the nascent carbohydrates, under the action of the silent discharge, in a manner precisely equivalent to the recent synthesis of an amino acid by Giamician, from acetone and prussic acid, under the action of sunlight

Whether the synthesis of amino acids in volcanic eruptions may ever be experimentally established, seems for obvious reasons extremely doubtful. Pütter¹ has shown that the waters of the Mediterranean, at some miles distant from the shore, contain organic substances, including amino acids, in small though measurable quantity, which led him to the conception of the sea as an immense (though very dilute) organic solution. Pütter conceives that these organic substances are due to the functional activities of the lowest marine organisms, the algæ and their like; but it may be noted that the organic products of vital activity are in general poisonous (ptomaines, etc.), and that in at least one known instance, the bottom of the Black Sea, decomposition products have in the absence of destructive bacterial activity made this "the world's most perfect desert."²

On the other hand, we know of organic products of inorganic origin (hydrocarbons), as some petroleums, and probably asphalt and the like.³ In the light of our new knowledge of volcanic chemism, there seems no good reason to deny the possibility of inorganic carbohydrates, and even nitrogenous compounds, as amino acids, etc.; and if for example it could be shown that the sea-waters in the vicinity of volcanoes contain a much higher percentage of organic substances than the sea generally, this view would receive considerable support.

Again, it is known that the Mediterranean, especially in the vicinity of Naples, is rich in marine forms, which led to the establishment there of the first marine biological laboratory. It is now known that the quantity of life in the sea is a function of temperature,⁴ and varies inversely with the temperature, contrary to popular ideas. If it could be shown that waters in the neighbourhood of active volcanoes are much richer in

¹ Stoffhaushalt, *d. Meeres*; *Zeit. f. allg. Physiol.* 7, 366, 1907. Pütter's work has, however, been recently repeated, and his results and conclusions flatly controverted by Henze: *Archiv. f. ges. Physiol.* (Pflueger), 123, 478, 1908.

² Daly, *loc. cit.*

³ Clarke, *Geochemistry*, 631, for lit.

⁴ Pütter, *loc. cit.* p. 338; Johnstone, *Life in the Sea*, p. 241, 1908.

animal life than other sea-waters at the *same temperature*, or if it could be shown that waters in volcanic areas show a marked increase in the quantity of animal life in the years following a volcanic eruption, we should have here further collateral evidence.

Nearly conclusive evidence would be gained if, following an eruption, these organic compounds were found in isolated pools formed in the downpour following the eruption, and free from any great quantity of the synthetic bacteria. But no such observations are known to the writer.

In general it may be remarked that the quantity of such substances would probably be small, and they would be mixed with other substances in a way that might readily mask their presence. Moreover, it might be only under special conditions, seldom realised, that they are precipitated into such isolated pools, in sufficient concentration to display their characteristic chemical reactions.

If, however, we conceive, with Spencer,¹ that the organic chemist in his syntheses is merely repeating, unconsciously, the processes by which these same substances have been built up in nature, and if, further, we are able to effect these syntheses in the laboratory under conditions which closely reproduce volcanic chemism, so far as our knowledge extends, we establish at least a working probability that such was the origin of these substances in nature. If now the present state of our geological knowledge warrants us in the conclusion that these conditions are impossible of realisation anywhere else, and that this was true throughout all geological time, this hypothesis gains as much strength as may be derived from the fact that any other mode of beginnings is inconceivable.

All geological evidence leads us to believe that volcanic activity has been prevalent, back to the earliest times (Geikie). We have evidence, moreover, that volcanic activity was at some periods far more general and violent than at the present time, as in the vast lava flows of the Snake River and the Columbia in North America, and more notably still in the Deccan, in India, where an area of over 200,000 sq. miles has been covered to a thickness sometimes exceeding 6,000 ft.

Although it is evident that these enormous masses were not erupted at any one time, but over a long period, they could

¹ *Prin. of Biol.*, 2nd ed. p. 700.

scarcely have united to almost solid blocks, unless the activity had been prolonged over a considerable period and involved rather wide areas. They could scarcely have been laid down without occasioning a considerable rise of temperature locally, and probably not without the formation of large quantities of carbon dioxide. Both of these would have contributed to increase the water-vapour content of the atmosphere with still further rise of temperature, vast cloud-formation, prolonged and violent precipitation, and accompanying electrical activity. Indeed, it seems not improbable that the implied conditions would in some degree approach, locally at least, the conditions imagined in the formation of a solid crust over a molten earth. It is possible that the different surface conditions conceivable under the nebular and the various meteoritic hypotheses would be differences of degree and generality rather than of kind, and that for our present purpose it matters little whether we accept one or the other, or regard them all as unsubstantiable at present.

There seems no more reason why such outbreaks should have taken place in Cretaceous than in Archean time. Neither does there seem any particular foundation for the long prevalent idea that the earth in the Archean period was entirely covered by water. The apparent fixity of the continental areas through known periods of enormous duration would itself suggest that there had been, even in the remotest ages, the recurrent projection of volcanic areas above the level of the sea. Possibly we have in the recent eruptions in the Bogaslov Islands a dim suggestion of primeval conditions;¹ and the anciently volcanic area in Yellowstone Park known as Hell's Half-Acre, with its boiling springs, its geysers, its gas-vents, and the varied mineralisation of its waters, may offer us a faint picture of an ancient day when a similar area, perhaps of much greater extent, was thrust up out of the Archean ocean.

¹ According to officers of the revenue cutter *McCulloch*, which returned from a cruise in northern waters, Mount McCulloch, which last year thrust its head up from the centre of Bogaslov Island, sixty miles west of Unalaska, has disappeared in the throes of another volcanic change. In its place, it is said, is a land-locked bay three miles wide, into which the cutter *Bush* sailed and made soundings. It was found that the water showed a depth of from eight fathoms at the edge to twenty-five fathoms in the centre. Mount McCulloch, which was first seen a year ago, when the cutter after which it is named arrived off the coast, had a height of 300 ft.—San Francisco dispatch, November 6, 1908.

We may readily conceive the formation of pools or even large lakes by such an upheaval. The temperature of these pools and lakes, in contact with the hot volcanic masses which formed them, might be high (cf. Observations made by revenue cutter *McCulloch*, 1907-8), so that by rapid evaporation the saline concentration might be raised far above that of the surrounding waters. Through the action of superheated steam on the rocks, this concentration, especially in metallic salts like those of magnesium, iron, etc., might be still further increased, and especially might we suppose, under action of carbon dioxide on phosphatic rocks like apatite, a considerable concentration of phosphoric acid.

Conceive now that these lakes or pools lay in the vicinity of volcanoes in prolonged activity, such as, for example, our present-day Stromboli. Even were the formation of organic substances and amino acids through the activity of the volcanoes extremely slight, this might, through continuous precipitation into these lakes, likewise acquire a considerable concentration, even to the point of forming a coagulum. This would cool in time, and might then, under ordinary atmospheric precipitation, acquire any desired degree of dilution. It might readily undergo a whole series of alternating coagulations and resolutions, so that it might repeat on a large scale very much the same series of reactions as are employed at the present time for the analysis and synthesis of these substances in the laboratory.

We should have thus, conceivably, a theatre offering almost every imaginable variety of conditions for inaugurating the initial stages of life. Precisely what these stages were we may not say as yet, for our knowledge of the mode of condensation, polymerisation, and the union among themselves of the amino acids, carbohydrates and hydrocarbons is still in the beginning. We have not the slightest reason, however, to believe that these processes are or were excessively complex, for the ways of nature are simple.¹

We have no reason to doubt that the processes were essentially the same as those by which the plant, and still lower organisms, build up organic substances from inorganic materials. We have every reason for supposing that these processes were in principle simply a reversal of those by

¹ Cf. Ciamician, *l.c.* p. 5, on simplicity of the plant's "laboratory."

which the compounds are obtained through the destruction of the protein molecule.¹

A great variety of syntheses of organic compounds has shown that the path to synthesis was plainly indicated in the methods, and stages, of analysis by which they were originally isolated. In the case of protein substances this process is, in foremost line, a simple hydrolysis. E. Fischer has shown that two amino acids may unite to form a more complex compound by simple dehydration. We may suppose that in the beginning it was the same.

These hydrations and dehydrations may or may not have taken place under the active influence of sunlight. For a time, under the dominance of the Helmholtz-Kelvin idea as to the age of the sun, it seemed as if the beginnings of life must have been in the dark, or at least in the diffused light of a nebular cloud. It is evident now, as Arrhenius² and others have shown, that these calculations had no decisive value. Insolation in the Archean period of the earth may have been even more intense than at the present time, or at least the chemically active portion of the spectrum may have been.

It seems highly probable that the beginnings were in the absence of free oxygen, and that oxidations came into play, as they do still, only at a later stage of organic complication,³ and after free oxygen had been introduced into the atmosphere through organic activity itself. This latter process, thanks to the labours of G. Bertrand, Wilstätter and others, we are probably now in a position correctly to conceive. Bertrand⁴ showed that in the case of the oxidase laccase, the active principle seems to be manganese, and that the activity of this ferment is directly proportional to the amount of the metal that it contains.

Apparently the office of the metal is to seize the oxygen atom at the moment of the dissociation, under the influence of sunlight or by other means, of the carbon dioxide molecule, and to hand this oxygen over, after a passing union, in a "nascent" state. In a similar way Wilstätter⁵ has shown

¹ Cf. Mann, *loc. cit.*; A. E. Taylor: *Univ. of Cal. Pubs.*, Pathol. Series, No. 9.

² *L.c.*, p. 61; also *Lehrb. d. Kos. Physik*, 1904.

³ Gautier: *Chimie de la Cellule vivante*, 1898.

⁴ *Compt. Rend.*, 122. 1132, 1896; 124. 1032, 1897.

⁵ *Liebig's Annalen*, 350, 48, 1906; 354, 205.

that the active principle of the chlorophyll of the green plant is magnesium (and not iron, as was long supposed), and in the light of W. Löb's and Fenton's syntheses of formaldehyde, we have reason to suppose that the office of the magnesium is similar to that of the manganese.

Following our conception of primeval conditions, we have then merely to suppose that the colloid particles, amino acids and their like, precipitated into a concentrated solution containing metal salts like magnesium, formed with the latter precisely such metallo-organic compounds as our organic ferments have been shown to be. Under insolation, or at a sufficient temperature and in a sufficient concentration, these primitive ferments would then begin what we may perhaps regard as the initial work of life, that of uniting the C or CO atoms of the dissociated carbon dioxide molecule to some other substance and setting oxygen free. In other words, it seems probable that these nascent forms of volcanic life themselves created the milieu or conditions for their own further evolution. This may have been the first of those seemingly marvellous "adaptations" or syndromes, with which living nature is crowded, and which are in reality "adaptations" only to our hopelessly myopic and strabismic anthropomorphism.

It is to be noted that this mode of carbohydrate formation may not have been the earliest of vital syntheses. There is much to suggest that it was not. The simplest of plants, we know, are already relatively complicated organisms. They are syncytia or if we like symbioses of the more primitive chloroplasts, which assimilate and divide even as does the cell itself, and are therefore, in the fullest sense, alive. We know of much simpler organisms, which are not plants in any sense of the word. They do not contain chlorophyll, they do not require sunlight, they do not assimilate carbon dioxide, they do not evolve oxygen. Some of these lowly organisms derive their carbon from carbonates,¹ and even from carbon monoxide,² one of the most deadly of known gases to the higher organisms. In the nitro- and nitroso-monads, the principal act of "nutrition" is a reaction to ammonia and to nitrous acid.

¹ Winogradsky, in *Lafar. Handb.* iii. 158, 1904.

² Kaserer : *Centr. f. B. u. P.* II., Nos. 22, 23, 1906.

It is readily conceivable that the earliest "organic milieu" was therefore a solution of carbonates, and the earliest of the ferments may have been therefore a combination of organic substances with, probably, a metal, able to promote the dissociation of the carbonate and the assimilation or union of the carbon with water or with peroxide of hydrogen, which is, as the researches of W. Löb and others indicate, perhaps an intermediary substance in all carbohydrate syntheses.

Whether the "nascent" carbohydrate is itself an enzyme or ferment for the assimilation of nitrogen from ammonia or other nitrogen compound, or whether here again a special enzyme intervenes, is as yet unknown. But the demonstration that ammonia is formed in quantity in volcanic action gives us every reason to suppose that these substances were originally present in sufficient concentration for the further synthesis of nitrogen compounds to amino acids to proceed.

There remains for consideration another substance which so far as we know is unconditionally necessary to vital chemism. This is phosphorus, or rather phosphoric acid. O. Loew¹ has shown that some primitive organisms may continue to live, but do not divide, in the total absence of phosphorus supply. The high percentage of phosphoric acid in the nucleins, the decisive rôle of the nucleins in cell-division, and its apparent prevalence in organic ferments, is further evidence of the importance of this element; and J. Loeb² has recently shown that the rate of nuclein synthesis in the fertilised egg increases in proportion to the number of nuclei already present.

Whether the synthesis of carbon and nitrogen, that is to say, of carbohydrates and amino acids, may be realised in the absence of organic compounds containing phosphorus is not as yet clear. It is certain, however, that one of the earliest stages in the complication of living substance is the phosphorisation of amino acids, that is to say, the formation of nucleins. The conditions for this addition have already been noted. The action of carbon dioxide on a phosphatic rock like apatite gives precisely such a solution of carbonates and phosphoric acid as would here be required. In the light of Loeb's results, with the formation of nucleins, substances

¹ *Chem. Energie d. lebenden Zellen*, p. 19, 1906.

² Boston address: *Science*, 26, 425, 1907. *Befruchtungsvorgangs*. Leipzig, 1908.

able to act as enzymes for their own synthesis, the formation of indefinite quantities of similar substance would, under suitable conditions, seem assured.

The successive stages of this organisation of living substance would then, in the dim light of our present knowledge, appear to be :

1. The reaction of carbon dioxide and water vapour in a concentrated mixture, under the influence of electrical discharges to formaldehyde, and perhaps also formic acid and other simple carbohydrates.

2. The reaction of these primitive carbohydrates, perhaps in their "nascent" state, that is in the moment of their formation, with ammonia or the amine or imine radicle, or with prussic acid, to form simple amino acids.

3. The precipitation of these colloidal substances into a concentrated solution containing metal salts, phosphoric acid, and doubtless other substances, and the combination of these amino acids with phosphoric acid on the one hand, with a magnesium-like metal on the other.

4. A series of coagulations and re-solutions resulting in various colloidal combinations, to the formation of ultra-microscopic particles of living matter, and the aggregation of these particles into primitive granules, microsomes, chloroplasts, and similar bodies.

5. The formation, through the chemical activity of these bioblasts, of an enclosing membrane, resulting in the first living "cells."

We may readily conceive such volcanic areas as we have imagined successively swept by tidal waves, by which these initial organisms would be washed into the ocean, or at least into enclosed seas, lagunes, and the like, there to undergo further modification by their new surroundings. The temperature of these early oceans, we have some reason to believe, was considerably higher, perhaps 20° or 30° C. higher, than at the present time. If this was not true generally, it might readily have been true locally over considerable areas.

Clausen, Hertwig, Loeb,¹ and others have shown that the rate of formation of living matter, that is to say, growth, is a function of temperature, and more or less follows the Arrhenius-van't Hoff law for the velocity of all chemical reactions, that

¹ Höber, *Physik. Chemie*, 322. 1902, for lit.

is, the rate is approximately doubled with each 10° C. increase in temperature. It seems probable from all this that the synthesis would have been rapid, and the capacity of the oceans to sustain life may very quickly have reached the saturation point. Estimating this period at not more than a few hundred or a few thousand years, and setting this over against perhaps fifty or a hundred million years which have since elapsed, we shall conclude that the quantity of life on the earth has perhaps been nearly constant throughout the whole range of geological time. There may have been some fluctuation through the reduction of the quantity of required ingredients to below minimal requirements. But from the known action of the sea in regulating the quantity of carbon dioxide in the atmosphere, variation from this source would scarcely have been great, nor have we much evidence for any great variation in the quantity of phosphoric acid and silica.

The evidence of wide variation in fossil life is scarcely to the point, since now as always the vast bulk of living substance is non-calcareous. In periods of glaciation, supposing the reduced temperature world-wide, there may have been considerable increase to even Arctic abundance. But in general it seems probable that life on the earth has varied rather in the forms of its transient aggregation than in volume.

NEW YORK, Dec. 1, 1908.

VACCINE THERAPY IN THEORY AND PRACTICE

By D. W. CARMALT JONES, M.A., M.B. OXON.

It has long been observed, that after recovery from an acute bacterial disease, a person is not liable to contract it again for a longer or shorter period. Such a person is said to be "immune" and to possess "active immunity." It is possible to produce immunity by artificial means, and two distinct methods are employed for this purpose, between which confusion sometimes arises. By one method an animal is infected with the disease in a modified form, and after its recovery its "immune serum" is drawn off and injected into patients with a view to conferring immunity upon them: such immunity is called "passive," and this is the object of sero-therapy. The other method consists in injecting into the patient the actual organisms, killed by heat, which cause the disease, and thereby conferring active immunity.

The latter method is known as "Vaccine therapy," and a vaccine, as used to-day, is a suspension of micro-organisms in sterile normal saline solution, containing a little antiseptic. The organisms have been killed by heat, their sterility is tested, and their number per unit of volume is correctly estimated.

It is this method of conferring active immunity which it is proposed to discuss in this article. Vaccine therapy has a far wider application than has serum therapy; it is easier of access and capable of accurate standardisation, and there can be little doubt that it will be the method of immunisation of the future. Serum therapy will not be further referred to here; it has indeed been previously dealt with in this journal by Dr. Inman.¹

In the past, artificial immunity has been induced chiefly for prophylactic purposes, in order that persons likely to be exposed to infection might be protected, but of recent years it has been introduced in the course of a disease as a method of cure.

¹ Inman, *Science Progress*, Oct. 1906.

The oldest and still the greatest prophylactic measure is vaccination against small-pox, and from this the whole method takes its name. Before this was introduced prophylactic inoculation against small-pox with matter from small-pox pustules was for some time in use. This was a common practice in the East long before it was introduced into England. It was observed in Constantinople by Lady Mary Wortley Montagu, wife of the British Ambassador, who subjected her own son to it. She introduced the method into England in 1721, in the teeth of bitter opposition, and her grandson during a severe epidemic made extensive use of it with brilliant results. He inoculated over two thousand persons, of whom only three died. Direct inoculation, however, was open to this serious objection—that although the patient himself suffered only from a mild attack, which was followed by immunity, he could none the less convey the infection in a virulent form to others; and the practice was finally prohibited by law.

In 1780 the attention of Edward Jenner was drawn to a vesicular disease of the udders of cows which sometimes infected the hands of dairy workers, who were found to be afterwards immune to small-pox. Instances are recorded of farmers who successfully inoculated their families. Jenner investigated the subject, and in 1796 inoculated a boy with cow-pox and afterwards with small-pox, to which he proved immune. He published his results in 1798, a long controversy followed, and in about 1800 the systematic practice of vaccination began. The results were immediately visible: in London in the two last decades of the eighteenth century the mortality from small-pox was 17,867 and 18,477 respectively, while in the first two decades of the nineteenth it fell to 12,534 and 7,856.¹

It is remarkable, in view of the wide employment and success of vaccination, that the causal organism of both cow-pox and small-pox is still not certainly determined, so that it is not possible to state definitely whether they are identical or not. It is probable that they are descended from a common origin, and that the organism is modified by passage through a refractory animal. The great principle established by Jenner is that it is possible, by using a modified *materies morbi*, to confer artificial immunity to an infective disease.

A century elapsed from the date of Jenner's first observations

¹ Councilman, article "Small-pox," Osler's *System of Medicine*.

before prophylactic inoculation was successfully carried out against any other disease. So long as the infective agents causing disease were unrecognised, experiments were quite dependent upon chance for success, and all recent advance in immunisation has depended upon a knowledge of bacteriology.

The first great step in this direction was taken by Schwann, who demonstrated that organic bodies do not undergo decomposition except in the presence of micro-organisms.¹ This was followed by the work of Pasteur on fermentation, and the identification of the yeast fungus as its cause,² in the year 1857.

The probability that disease was caused by similar organisms was by this time suspected ; indeed, in 1850 Davaine had observed and described anthrax bacilli in the blood of animals with that disease,³ though he did not recognise them as the cause. In 1860 Delaford cultivated the bacteria in blood, and in 1863 Davaine showed their constant presence in anthrax blood, and suggested that the disease was due to them. By this time, then, a specific organism had been shown to be constantly present in a certain disease, and to be capable of cultivation outside the body.

It still remained to obtain the organisms in pure culture, which was first done by Koch in 1876, by growing them on a solid medium. This work was confirmed and continued by Pasteur ; both these observers produced the disease in animals.

In 1881 Pasteur first used a vaccine made from attenuated anthrax cultures, with which he successfully inoculated sheep. The method of Jenner was thus repeated, employing an organism whose relation to the disease against which prophylaxis was sought was definitely known. The extension of this principle to other diseases was now merely a matter of time.

In 1885, after many experiments on animals, Pasteur inoculated the first human patient against rabies. In this again no organism had been isolated, but the virus had been traced to the spinal cords of infected animals, and Pasteur employed emulsions of such cords. Inoculations are used after a person has been bitten by a possibly rabid animal, but only as a prophylactic measure. The incubation period of rabies is a long one—three weeks or more—and it is possible to confer an active immunity before the infective agent reaches the spinal cord.

¹ Ehrlich, Croonian Lecture, *Proc. Roy. Soc.* vol. lvi.

² Bosanquet, *Serums, Vaccines and Toxins*.

³ Bosanquet, *op. cit.*

If symptoms have once occurred, treatment is of no avail. Statistics of the value of the treatment are difficult to obtain, but it is stated that before inoculation was adopted at least ten per cent. of persons bitten by rabid animals succumbed, while since 1886 at the Pasteur Institute the mortality has always been under 1 per cent. in any year, and in every year more than a thousand cases have been treated.¹

In 1888 a new series of observations were made which threw much light on the problems of immunity. In this year Roux and Yersin discovered diphtheria-toxin, a product of the diphtheria bacillus, and Kitasato discovered tetanus toxin; and in 1890 Behring discovered in the serum of animals immunised against diphtheria and tetanus, substances which neutralised these toxins, which he called "anti-toxins." This was the foundation of serum therapy.

In 1890 Koch introduced his "tuberculin," a product of the growth of tubercle bacilli. This was intended for use both as a means of diagnosis and as a cure for tubercular infection. It was not a vaccine, but was used as "toxi-therapy." It was abandoned by Koch in 1897 for other preparations—tuberculin O. and tuberculin R.—which are extracts of the bodies of the bacilli. Tuberculin R. acts as a vaccine. Some very unfortunate results attended the early experiments, and the method fell into disuse; but with accurate methods of estimating the effects of various doses, tuberculin R. has become a remedy of the first importance.¹ Recently a "bacillary emulsion" has been introduced, which approximates still more closely to ordinary vaccines.

Further notable experiments in prophylactic inoculation are those of Ferran and also Haffkine with cholera, and of Haffkine with plague. In 1897 Haffkine inoculated himself with a sterilised bouillon culture of the bacillus pestis, and it has since been extensively used in India. Statistics show that a partial immunity is conferred by this treatment.

The most important prophylactic measure of recent years is that introduced by Wright against typhoid fever. Some large figures are available by which to judge of its efficacy. During the South African War about 19,000 men in South Africa and India were inoculated. Of these 1 in 84 was infected, with a case-mortality of 17 per cent. At the same time, among

¹ Bosanquet.

about 150,000 unprotected men, 1 in 40 took the disease, with a case-mortality of 25 per cent. That is, the risk of infection was reduced by one-half, and if infected, the chance of death was reduced from 1 in 4 to 1 in 6.¹

Hitherto, as we have seen, vaccine treatment has been almost wholly prophylactic, and to Wright belongs the distinction of having first exploited it in a scientific manner as a curative measure. Wright first realised that every organism which causes localised disease and is capable of pure cultivation may be employed as a vaccine to cure the disease it causes. He further showed that elaborate methods of attenuating growth are unnecessary, and that the organisms have merely to be grown on solid media, washed off in saline solution and sterilised by heat, and he has introduced an accurate method of standardisation. The result of this has been to introduce vaccine therapy into every-day practice, so that it is possible to bring against any bacterial infection a specific means of attack, and the patient's natural powers of recovery can be stimulated, and cure hastened.

In 1902 Wright published his first experiments in curative inoculation. These were made on some refractory cases of staphylococcal infection which he had had under treatment during the two previous years. In the same paper he suggested the extension of the method to other organisms, and from that time the work has gone forward, till at the present time it is possible to prepare a vaccine for any infection of which the organism can be found.²

It is difficult to overestimate the importance of these advances. Without specific methods of this kind the physician is powerless to deal directly with any bacterial infection; the best that is open to him is to put his patient under the most hygienic conditions, and leave him alone to establish his own immunity if he can. Vaccine therapy, which offers a rational means of producing immunity, may fairly be considered the greatest advance in the treatment of disease since Lister introduced antiseptic surgery.

Exponents of therapeutic immunisation claim, then, that their method is a rational and scientific attempt to deal with bacterial infections. It is fair to inquire on what grounds,

¹ McCrae, article "Typhoid Fever," Osler's *System*.

² Wright, *Lancet*, March 29, 1902.

other than purely empirical ones, this claim is based. For this purpose it is necessary to study a little more closely the fact of natural recovery. This is a very remarkable phenomenon when what it implies is considered. When a person "catches" such a disease as typhoid fever, it is reasonable to suppose that no very large number of typhoid bacilli are introduced into his system; but in the course of the disease these become exceedingly numerous, and can be recovered from his blood in enormous numbers, and yet recovery occurs in 90 per cent. of cases. That is to say, that a person, infected when in good health, cannot prevent a few bacilli multiplying into many, and yet, although weakened by the disease, he is ultimately able to get rid of the larger number. The first suggestion is that the bacilli have exhausted all the nutrient material in the patient's body; but since they can be readily cultivated on the tissues of a convalescent patient, this view is untenable. The alternative suggestion is that the patient's blood has the power of elaborating some substances which are inimical to the bacteria.

It appears to be true that the blood has the power of forming anti-bodies to any foreign albuminous substance which is introduced into it. This is the fundamental hypothesis upon which all immunity depends, and it is this which is exploited in vaccine therapy.

The truth of this has become plain through a large number of experiments, an account of some of which will make the conception clear. If ricin be added to blood, the red corpuscles are agglutinated and destroyed. Ricin has an affinity for the red blood-corpuscles, and is therefore called an "hæmotropic" poison. If now ricin be injected in small but increasing doses into an animal, the serum of this animal in course of time develops such properties that, if it be added to ricin, the ricin loses its property of agglutinating red corpuscles. It contains a "ricinotropic" substance, which combines with the ricin, and so forms an inert body incapable of agglutinating red corpuscles.¹

Again, if the red blood-corpuscles of an animal of one species, for instance a goat, be mixed with the serum of an animal of another species, for instance a sheep, the goat's corpuscles will remain intact. But if goat's corpuscles are

¹ Wright, *A Short Treatise on Antityphoid Inoculation*.

injected in gradually increasing doses into a sheep, the sheep's serum develops "erythrocytotropic" or "hæmolytic" substances, which dissolve goat's corpuscles, so that if this serum be mixed with goat's corpuscles they become dissolved and a clear solution of hæmoglobin is obtained. The serum will only cause hæmolysis of sheep's corpuscles, or those of some species nearly allied: this is important, as it is an instance of the specific nature of anti-bodies.¹

If milk be injected into an animal in progressive doses, its serum develops "galactotropic" substances which coagulate milk, and the injection of spermatozoa or of ciliated epithelium leads to formation of anti-bodies in the serum which arrest the movements of these cells. The formation of anti-toxins to the toxins of diphtheria and tetanus has been referred to; they are developed after injection of the toxins in progressive doses into horses, whose serum is drawn off and standardised. This is so well established as to be a matter of commercial enterprise.

This property of the blood of forming anti-bodies is the explanation of the phenomenon of recovery from disease. Virulent bacteria are introduced, and gradually the blood forms bacteriotropic substances which exercise some hostile influence over the bacteria. The object of vaccine therapy is to stimulate the blood to form anti-bodies to a given bacterium by injection of the same organisms in a dead condition, and so incapable of further infecting the patient.

The rationale of this as a prophylactic measure, to provide a person with a given set of anti-bodies before exposure to infection, is easily understood. Curative inoculation requires some further consideration. The obvious criticism is that it must be useless to introduce more organisms, dead or alive, when the body already contains more than it can deal with.

The reasons for curative inoculation come out of the following considerations. Bacterial infections are of two kinds—general, or septicæmic, and local. In the septicæmias the organisms are in circulation in the blood, and may sometimes be cultivated from it; but in local infections the bacteria are confined in certain areas, such as one or more joints or glands, and none can be recovered from blood cultures. The local infections are those which give the best results from vaccine therapy, and this is because in these cases the circulating

¹ Macleod, in *Recent Advances in Physiology and Biochemistry*.

blood is poor in protective substances. The presence of these protective substances in the blood can be demonstrated, and their amount measured by methods introduced by Wright and Douglas.¹ These observers estimate the amount of phagocytosis which occurs when white blood-corpuscles, bacteria, and blood serum are mixed together.

Phagocytosis is the power of ingesting foreign bodies which is possessed by white blood-corpuscles (leucocytes). It was first observed by Haeckel in 1862, in the leucocytes of a mollusc.² Leucocytes can ingest bacteria, and it was long contended by Metchnikoff, and his school, that immunity depended on this power. The experiments of Wright and Douglas proved that something further is required. If leucocytes are washed free of their serum and mixed with bacteria, phagocytosis does not occur, or only to a very small extent. If to this mixture the serum of a healthy man is added, the leucocytes will ingest a number of bacteria, and an average per leucocyte can be worked out.

Phagocytosis depends on some property of the serum, some substances which act on the bacteria so as to render them capable of ingestion by leucocytes. Wright has called these bodies "opsonins." If the experiment mentioned is repeated, substituting for the serum of a healthy man that of a person suffering from a local infection by the bacterium in question, it is generally found that his serum has less power of preparing bacteria for ingestion, and that a smaller number of bacteria are taken up by the phagocytes. In practice the average number ingested after using normal serum is regarded as unity, and the number ingested after using the patient's serum is expressed as a fraction of the normal. This fraction Wright calls the "opsonic index."

By this means it is possible to estimate the patient's resistance to the invading organism, and whether benefit is likely to follow the exhibition of a vaccine. The explanation of the low index in local infections is that the focus is to some extent shut out of the blood stream, that the protective bodies have been exhausted in their unsuccessful attempt to destroy the focus, and that bacteria do not pass into the blood and stimulate the formation of anti-bodies. It is believed that these

¹ Wright and Douglas, *Proc. Roy. Soc.*, vol. lxxii.

² Metchnikoff, *Leçons sur l'Inflammation comparée*.

anti-bodies are formed in the subcutaneous connective tissues,¹ and the correct treatment in such a case is to inject a suitable dose of the appropriate vaccine under the skin. This is followed by an outpouring of anti-bodies into the blood, a rise in the opsonic index, and clinical improvement. The result of a dose may be determined by observations on the opsonic index. The inoculation is followed first by a fall in resistance, called the negative phase, and if the dose is correct this is succeeded by a marked rise and a gradual fall. If the dose is too large the negative phase becomes pronounced and the patient changes clinically for the worse. It is on these principles that inoculation is employed in localised infections.

With regard to the septicæmias the case is slightly different. General infections, compared to local, are rare, because as a rule any organisms which enter the blood are killed off at once by the anti-bodies normally present. But when bacteria survive these and succeed in multiplying in the blood it appears that fresh anti-bodies are not formed until the bacteria are deposited from the blood in the proper tissues which subserve this function. In some such cases a vaccine may be advantageously used, but it must be borne in mind that the condition is one of extreme gravity, and that the production of a negative phase of any magnitude may well lead to a fatal result. Minimal doses only must be used, and their effect must be scrupulously watched by estimation of the opsonic index.

The first paper on the opsonic index was published in 1903, and the subject has been exhaustively investigated by numerous workers ever since, and has excited not a little criticism. It is not proposed to enter into any detailed defence or criticism of the method in this place. All advances and variations of method introduced in recent years have been made in deference to its indications, and its general accuracy is assumed to be established.

At the same time it is admitted that it is not universally accepted, and that some able workers have failed to obtain consistent results with it. A few only of the most striking evidences of its reliability will be given here, most of which have transpired in Wright's laboratory. In that laboratory the method is used for practical purposes only. It is employed as a guide in treatment, and has been so employed for the last

¹ Wright, *Lancet*, Aug. 17 and 24, 1907.

five years, and patients treated on these principles have given consistently satisfactory results. The element of personal bias is eliminated, because the estimations are made by observers who do not know what cases they are investigating: this is not insisted upon, but in practice each worker undertakes haphazard a certain proportion of the day's work. And the total estimations made now number tens of thousands.

The most important points in connection with vaccine therapy will best be made clear by a chronological survey of its development, as recorded in papers published by Wright and his fellow workers. This method is selected because of the authoritative character of these publications, and also because the present writer has some personal experience of the observations.

After their early observations that phagocytosis of bacteria by leucocytes did not occur except in the presence of serum, Wright and Douglas next proceeded to experiment with different varieties of bacteria, and demonstrated that the opsonic effect was produced on almost all; two only, and those nearly allied, out of ten organisms examined, proved refractory.¹

The *staphylococcus pyogenes* was then examined in detail, when it was found that persons suffering from pustular infection showed a lower opsonic index to this organism than healthy persons. Further, the pus derived from boils proved to have less opsonic power than the blood. The patients were inoculated with staphylococcus vaccines, and it was found that their opsonic indices rose, concurrently with clinical improvement of the lesions.²

The tubercle bacillus, the next investigated, presented great technical difficulties, but when these were overcome, the same facts became plain. The indices of patients with localised tubercular disease were found to be lower than those of healthy persons, and the indices obtained from pathological fluids, such as serous effusions, were found to be lower still. Some cases of localised tuberculosis were treated with inoculations of new tuberculin, and considerable improvement was recorded.³

If these methods were to be of use in the diagnosis of tubercular disease, it was clearly necessary to find out if normal persons offer the same resistance to the tubercle bacillus—that

¹ Wright and Douglas, *Proc. Roy Soc.*, vol. lxxiii.

² *Ibid.* vol. lxxiv.

³ *Ibid.* vol. lxxiv.

is, if a normal index really exists. Bulloch accordingly investigated the opsonic index of sixty-six healthy persons, regarding himself as normal; he found an average of '95, the variations ranging from '8 to 1'2, and concluded that the sera of normal individuals are almost identical.¹ Very numerous observations on the tuberculo-opsonic index followed these researches; and a fact which was soon elicited was that, while patients with strictly localised tuberculosis have consistently low indices, those who suffer from constitutional disturbance have variable indices, which may be either above or below normal, and it was concluded that such patients live in a constant succession of negative and positive phases, due to the entrance of bacilli into their blood from the focus of infection, and consequent "auto-inoculation." At this time a case of cystitis was examined which presented the appearance of tuberculosis; the indices yielded were consistently normal, and no tubercle bacilli were ever found. The case was declared not to be tubercular, and was ultimately cured by the use of a vaccine made from another organism isolated from the urine.²

The possibilities and limitations of vaccine therapy were recognised by Wright from the first, and he very early drew attention to the negative phase and the possibility of increasing it. A correct inoculation leads to a short negative phase, a steep rise, and a gradual fall to a level higher than that at which inoculation was begun; this is described as the "Law of ebb, and flow, and reflow, and maintained high-tide of immunity."³

In early experiments in immunisation, without access to any delicate means of recording the results of inoculation, it was considered necessary to produce some degree of constitutional disturbance to ensure the formation of anti-bodies. One of the early results of observations on the opsonic index was to show that this is unnecessary, and that a satisfactory immunising response can be excited without discomfort.⁴ Dosage was accordingly reduced. In the earliest cases published by Wright the doses varied from $\frac{1}{2000}$ to $\frac{1}{125}$ mg. of T.R., but in 1906 he recommended the use of doses between $\frac{1}{1000}$ and $\frac{1}{3000}$ mg., and at the present time doses as small as $\frac{1}{20000}$ mg. are in frequent

¹ Bulloch, *Trans. Path. Soc. Lond.*, vol. lvi.

² Wright and Reid, *Proc. Roy. Soc.*, vol. lxxvii.

³ Wright, *Clinical Journal*, Nov. 9, 1904.

⁴ Wright, *Trans. Roy. Med. Chi. Soc.*, 1906.

use. Different figures from these appear in the original papers owing to a misconception as to the actual strengths of preparation used ; the figures here given represent the doses as compared with those now employed.

From the data collected from observations on the opsonic index many facts with an important bearing on practical treatment have become apparent. In the case of local infections it is clear that the blood is poor in anti-bacterial substances, yet even this poor supply is sufficient to prevent the bacteria living in the blood stream. The foci of infection themselves are always found to be areas of "lowered bacteriotropic pressure," the anti-bacterial substances being exhausted by the bacteria with which they first come in contact. From this the indications for treatment are clear : the first is to raise the opsonic content of the blood by means of vaccines, and the second is to determine the blood when enriched to the site of infection. Many means may be adopted for this purpose : either a passive hyperæmia may be produced by constriction of a limb above an infected joint—the method of Bier—or active hyperæmia may be excited by heat or massage or physical exercise. All these methods bring blood to the site, and a non-viscid lymph of high permeating power may be produced by the ingestion of citric acid ; this facilitates the determination of blood to any congested area.

In the case of an unhealed sinus, lymph may be drawn through it by the use of a lotion which contains citrate of soda, to prevent coagulation, and salt in greater concentration than it is in the lymph to induce a flow of liquid outwards. Stagnant fluids in foci should be removed by mechanical means.

Bier had long used passive congestion in the treatment of chronic arthritis, and had observed that joints other than the one congested improved. This is due to the flooding of a focus of infection with lymph, and thereby washing bacteria into the blood stream and so setting up an auto-inoculation. The possibility of exciting auto-inoculation, which had been long recognised in cases of constitutional disturbance, was first observed in localised disease by Freeman.¹ This observer, while investigating a case of gonorrhœal arthritis, found one day a marked rise in the patient's index to gonococcus following massage of the affected joint. An extended series of observations followed, which demonstrated that this phenomenon occurs also

¹ Wright, *Trans. Roy. Med. Chi. Soc.* 1906.

after operations, exercise, after deep breathing in the case of lung disease, and after reading aloud in the case of disease of the larynx.

This discovery of the possibility of inducing auto-inoculation in cases of local disease has proved to be of the greatest importance in vaccine therapy for several purposes. It can be employed as a crucial test in diagnosis, it offers a most valuable means of treatment in certain cases, and it is necessary to bear it in mind in order to devise correct treatment for patients suffering from constitutional disturbance. For the purpose of diagnosis of an obscure lesion, in a site such as a joint, the patient's blood is drawn after a period of rest, and the joint is then subjected to some treatment which induces hyperæmia, such as massage or exercise, and the blood is drawn again at varying periods afterwards. If after this treatment a definite variation is found in the opsonic index to any organism, in either direction, that is strong presumptive evidence of infection by that organism—it is indeed evidence which is the next best thing to the actual demonstration of the microbes in the lesion. It is of especial value in making a differential diagnosis between two possible infections—as, for instance, gonococcus and tubercle. If hyperæmia produces evidence of auto-inoculation with one, and no alteration with the other, the experiment may be considered to afford conclusive proof. This is no matter of theory—it is a practical method in constant use; and several charts have been published which bring out the points in cases which afterwards improved when treated with the vaccine thereby indicated.

Some recent work in the use of auto-inoculation will be referred to later, but some points that require consideration in the choice between auto-inoculation and vaccine treatment may be discussed here. In any given case auto-inoculation insures the use of the correct organism, the physician is not limited by his facilities for cultivating the microbe, and treatment may be begun at once without preliminary study. But there are certain disadvantages in its use. The organisms thus introduced into the blood are living, and capable of spreading infection; they are introduced in unmeasured doses, the lesions may not be conveniently situated for the application of the necessary stimulus, and it is generally found that the patient undergoes a maximum of intoxication for a minimum

of immunising response; and a last, but important practical point is that the method makes great demands on the time both of the patient and his physician.

The study of auto-inoculation suggests certain indications in the treatment of disease with constitutional symptoms; these symptoms are the result of auto-inoculations of unknown strength, and until these are stopped the use of measured doses of vaccines cannot be advantageously employed. The guiding principle in pyrexial conditions, then, must be to secure rest in order to put a stop to auto-inoculation, and then to adopt properly controlled vaccination.

This applies especially to the treatment of pyrexial phthisis. All exercise, every fit of coughing, even breathing, induce an auto-inoculation, and the lungs can never be completely controlled. The indication is for the most complete rest, the effect of which in the reduction of temperature is well known. When this is successful it may result in converting the general into a strictly local disease; and if this is brought about, it may then be possible to initiate treatment either by auto-inoculation or vaccine therapy. It has been suggested that where it is desirable to induce a lymph flow through any focus, this may be assisted by the ingestion of citric acid to decrease the viscosity of the blood. Similarly, when auto-inoculation is to be checked, this will be helped by ingestion of calcium, either as milk or otherwise, in order to increase the coagulability of the blood, and to make it thicker.¹

It will be clear from the foregoing that the cases which are best adapted for inoculation treatment are strictly localised bacterial infections; that when there is evidence of general infection every effort must be made to check auto-inoculation, and to reduce such an infection from a general to a local one. Having done this, inoculation should be undertaken with appropriate vaccines, in suitable doses and at suitable intervals: inattention to these points will give unfavourable results. As the opsonic content of the blood is raised it should be determined to the focus of infection by some physical means; stagnant fluids should be removed mechanically, and coagulation in sinuses prevented; and, if necessary, the fluidity of the blood may be increased by the use of citric acid.

With regard to the treatment of general septicæmia, in

¹ Wright, *Lancet*, Aug. 17 and 24, 1907.

diseases which are running a normal course towards recovery there is no advantage in inoculation, and in excessive toxæmic conditions the addition of the least overdose of vaccine may lead to disaster; but in cases of septicæmia which show any sign of becoming chronic, or in desperate cases, it is possible that the introduction of vaccine into the subcutaneous tissue may lead to the elaboration of anti-bodies and clinical improvement.

The case for inoculation treatment may fairly be considered to be made out; but theories of exceeding beauty do not always give satisfactory results in practice, and the question that concerns practical people is whether patients really do improve under vaccine therapy or not. The answer is that the results are exceedingly satisfactory. The day has long gone by when there was any need to publish as novelties cases successfully treated by vaccine therapy. When the method was first introduced Wright published several series of cases, and showed particular patients at various societies; and some of these may be briefly recalled. In 1904¹ a series of twenty cases of acne, furunculosis and sycosis was published in detail; these had been treated with a staphylococcus vaccine, and had showed marked improvement; and in 1906 a long list of cases of localised tuberculosis was published in an important paper read before the Royal Medico-Chirurgical Society, and several of the patients were shown.² The cases included a large variety of lesions, and several specimens of each lesion. Some cases of lupus were shown, all of which had undergone some improvement, but in only one had a cure been effected. Five cases of tubercular glands were all cured, seven cases of severe tubercular ulceration of subcutaneous tissues had improved greatly, and five cases of infection of the genito-urinary system all showed marked improvement.

One case must suffice as a specimen; that which follows is chosen because the patient's subsequent history is known up to the present time. A man, aged thirty, developed tubercular glands in the neck and an abscess at the point of the shoulder in 1902. In 1903 he was operated on, but the wound did not heal, and five further operations were performed during the year. In December the whole skin from the left ear to the left shoulder was ulcerated, with a deep ulcerated crater below

¹ Wright, *B. M. J.*, May 7, 1904.

² Wright, *Trans. Roy. Med.-Chi. Soc.*, 1906.

the ear, and there was a large gland in the axilla. Inoculation treatment was then undertaken with new tuberculin and a staphylococcus vaccine, and the local application of formalin and gelatine. In eleven months the whole skin was completely healed except one area the size of a threepenny-piece. Six months later further glands became enlarged and broke down, treatment was resumed, and the lesions completely healed. From that date to the present time the patient has kept in very fair health, and though not a strong man he is able to be constantly at work, and is now employed in the laboratory.¹

The limitations of vaccine therapy are at once evident from a consideration of the mechanism of immunisation. It is not a remedy which is being applied, but a stimulus to the body to form its own remedies. On any given occasion the body may fail to respond to this stimulus, and if this occurs, immunisation does not result. Further, the protective substances may disappear from the blood, or the results of a therapeutic dose may be upset by auto-inoculation. Lastly, the physician may fail to determine a flow of lymph to the focus of infection; this is the reason of the lack of complete success in the treatment of lupus. It is very difficult to maintain an adequate blood supply to the outer layers of the skin, which explains the difficulty constantly experienced in treating superficial skin infections.

It is clear that in vaccine therapy the physician has in his hands a powerful weapon for good or ill, so powerful that its use can hardly be wisely undertaken without some accurate means of gauging the results of an inoculation. This may be done in various ways: by elaborate experiments on animals with living cultures, by observation of the toxic effects on patients, by observation of the clinical symptoms, and by measurement of the anti-bacterial substances in the blood.

Experiments on animals may be left out of account as impracticable, and toxic effects are often imperceptible, and cannot be expressed in any accurate terms. The choice lies between observation of the clinical symptoms and estimation of the anti-bacterial substances in the blood.

In some infections, such as those due to staphylococcus, the conditions are very easily gauged from the clinical picture. If a patient has a boil, and is inoculated with staphylococcus, and next day the boil is gone, it is clear that the dose received

¹ Wright, *Trans. Roy. Med.-Chi. Soc.*, 1906.

was the correct one. If the boil is worse, the dose was probably incorrect; and even when this has happened no really serious damage is done, and the physician may fairly accept the risk. But in other infections, such as tubercular disease of joints, in which weeks may elapse before any marked clinical change is produced, either for better or worse, it is essential for the physician to have some other guide as to whether he is doing right or wrong. Some observers, notably Latham, claim that in phthisis the temperature is an accurate guide, and Latham has published a series of double charts with curves of the opsonic index and of the temperature, and these curves vary inversely in a very remarkable manner.¹ He argues from these that a high temperature means a low index, and that dosage and spacing of inoculations may be regulated thereby. This leaves out of account the possibility of high temperatures being due to infections by other organisms—a possibility not to be neglected. Moreover, other observers have published similar charts in which the two curves run almost precisely parallel, and others again in which no correlation can be traced.² Observations are not yet sufficiently numerous, nor in sufficient agreement, to allow of the temperature being considered a reliable guide. Broadly speaking, it is clear that if the principles of vaccine therapy are sound, clinical improvement must be accompanied by a rising opsonic index; but to say that at any given moment the height of the index can be inferred from the clinical condition is to deny that this condition can be influenced by anything but an immunising response.

It appears, then, that it is necessary to estimate the anti-bacterial power of the blood. Now the blood is known to contain a considerable number of different anti-bacterial substances, and the question arises whether all of these are to be estimated, or whether the condition of one may be regarded as a measure of the whole. The blood has been shown to contain different bodies which agglutinate, kill, dissolve, and "opsonise" bacteria. The attempt to estimate the whole of these would clearly occupy a great deal of time; secondly, we have at present no means of demonstrating the action of certain of these bodies against certain bacteria—for instance, the bactericidal power of serum can be demonstrated against

¹ Latham, *Proc. Roy. Soc. Med.*, April 1908.

² Wright and others, *Lancet*, Nov. 2, 1907.

typhoid, but not against staphylococcus; and thirdly, there may be many other anti-bacterial agents in the blood which we have no means of demonstrating at all. It is therefore impracticable to completely estimate all the anti-bacterial properties of the blood, and we have to make a final consideration as to whether a partial estimation is of value.

The protective substances which are most readily estimated are the opsonins. Their influence is exerted on nearly all bacteria; the technique is a comparatively simple one—no delicate quantitative estimation of this kind can in any case be carried out without considerable skill and great care—and in skilled hands a very high degree of accuracy may be attained in the estimation.¹ Its value, however, depends on the establishment of a general correlation between the clinical condition and the condition of the index. It is submitted that the large number of charts now published have established such a correlation, and that a method which has brought to light facts of the practical value of auto-inoculation, and by means of which dosage has been so regulated that a pronounced negative phase is hardly ever seen, is its own defence. One more specific instance may be given. In Wright's laboratory blood samples from two or more workers are used as controls, and it is found that the counts of such blood are always in agreement within narrow limits. On no less than three occasions it has happened that persons, whose blood had at first been normal, became abnormal, and in each of these cases a tubercular lesion was afterwards clinically discovered.²

With regard to recent work, more and more organisms have been exploited for vaccine treatment, and in the last few years a most interesting use has been made of auto-inoculation in the treatment of phthisis at Frimley, the convalescent branch of Brompton Hospital.

Paterson, following Walther of Nordrach, has employed exercise in the treatment of phthisis. He has elaborated a most careful graduation of labour, from a half-mile walk up to full navy work. He has used the temperature as a guide, and on a rise to 99° work has been stopped and the patient has been put to bed. It has been noted that after such a rise there has sometimes been a marked improvement in the

¹ Fleming, *Practitioner*, May 1908.

² Wright and others, *Lancet*, Nov. 2, 1907.

patient's condition. The results of this treatment have been highly satisfactory: two years after discharge 119 patients out of 164 were found to be at work.

Inman investigated the opsonic index in cases under this treatment, demonstrated that a series of auto-inoculations were being induced, and showed that in some cases no amount of work produced any influence on the index. Such patients were considered to be cured.¹

Latham has recently introduced an innovation in the method of administration of vaccines. On the ground that there is reason to believe that pulmonary tuberculosis is generally due to infection through the alimentary tract, he gave first tuberculin and afterwards other vaccines by the mouth.² Roughly, twice the ordinary hypodermic dose is given, with the addition of fresh horse serum, which promotes absorption and appears to have other therapeutic value. It is essential to give the vaccine on an empty stomach. A satisfactory immunising response is found to follow the administration. It is not clear that this method has any advantage over the hypodermic; with a sharp needle the operation itself is the merest trifle, and the vaccine can be introduced direct into a suitable site. By the indirect method it is impossible to guarantee absorption, and it seems to be a reactionary step to work in the dark in preference to the light.

Wright long ago expressed his belief that the physician of the future will be an immunisator; it would seem that the physician of the present is becoming one, but this method has not yet taken the place in medicine which is its due. We are still only at the beginning of things; the door is open, but we are as yet only on the threshold; there is research for all who seek it in entirely unbroken ground, there is means of treating cases hitherto deemed incurable. As surely as the principles introduced by Lister dominate surgery, so surely will vaccine therapy dominate the treatment of infective disease.

Author's Note.—I have to acknowledge the kindness of Sir Almroth Wright, M.D., F.R.S., in allowing me to use the proofs of his collected papers, now in the press, for the compilation of this article.

¹ Paterson, also Inman, *Lancet*, Jan. 25, 1908.

² Latham, *loc. cit.*

ISOMERIC CHANGE

PART I.—HISTORICAL DEVELOPMENT OF THE THEORY

By T. MARTIN LOWRY, D.Sc.

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THE years that immediately followed the formulation of the Atomic Theory were characterised by great energy in the determination of equivalent or atomic weights, and by a more zealous study of the analytical composition of the most various substances than at any previous period in the history of chemical science. The analysis of mineral bodies, such as the metallic oxides and chlorides, presented, relatively speaking, only slight difficulties, and it was no very serious task for Berzelius to show that the three differently coloured oxides of lead differed from one another in the relative proportion of lead and oxygen, the brown peroxide containing twice as much oxygen as the yellow litharge; or that differences between the two oxides of sulphur, repeated in the sulphites and sulphates derived from them, were due to a similar inequality in the proportions of combined oxygen in the various compounds. Similar instances were multiplied to such an extent that a widespread conviction was established that in all cases differences of properties might be attributed to differences of chemical composition.¹

Polymorphism.—It was characteristic of this attitude of mind that Haüy, in his classification of minerals, laid down the law that each mineral substance possessed one specific crystalline form, which could be assumed by no other species, and thus denied at one stroke the possibility both of isomorphism and of polymorphism. The supposed identity of composition of calcite and aragonite was dismissed—in strict accordance with current

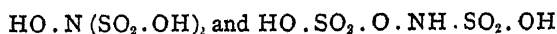
¹ For the early history I am indebted to F. P. Armitage, *A History of Chemistry*; to Prof. Armstrong's article on "Isomerism" in Morley and Muir's *Watts' Dictionary*; and to Roscoe and Schorlemer's *Treatise*, to which reference may be made if it is desired to trace out the original literature.

ideas as to the intimate relationship between physical properties and chemical composition—by the suggestion that the dimorphism was due to the presence of iron in the calcite and of strontia in the aragonite crystals. It is true that this assumption was soon found to be false—strontia being by no means a necessary constituent of aragonite, nor iron of calcite—but the possibility of dimorphism was only regarded as fully established when Mitscherlich, in 1823, discovered that similar differences of crystalline form could be produced artificially, in materials of identical origin and of established purity, by crystallising sulphur under slightly varied conditions.

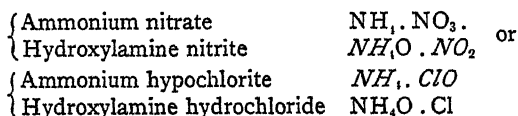
Allotropy.—The fact that diamond gave rise to carbonic acid when burnt was established qualitatively by Lavoisier, and quantitatively by Tennant in 1796, whilst Davy in 1814 proved the absence of moisture from the products of combustion, and therefore of hydrogen from the diamond itself. But the allotropy of charcoal and diamond, and still more so that of charcoal and graphite (established by Mackenzie in 1800), was complicated by the invariable presence of a greater or smaller proportion of ash, and had the supreme drawback that the various allotropes had to be accepted as natural products and could not be converted into one another, nor even separated from their impurities, by artificial methods. As a matter of fact, if the slight differences resulting from dimorphism be put on one side, the first clear case in which inorganic materials of identical composition were proved to be capable of existing in widely different forms was only forthcoming (long after the problem had been solved in another field of work) when Schrötter in 1845 (*Pogg. Ann.* 81, 276) demonstrated the chemical identity of red and yellow phosphorus, and Andrews, eleven years later (*Phil. Trans.* 1856, p. 13), proved that ozone consisted of oxygen and of oxygen only.

Isomerism of Inorganic Compounds.—It is noteworthy that both these illustrations are derived from the behaviour of elementary substances, and that the observed differences are known to be associated with changes in the size of the molecule. If this source of variation be excluded, the task of finding amongst inorganic materials substances in which identity both of composition and of molecular complexity is associated with marked diversity of properties becomes a very formidable one, and even at the present time the number of cases known is so small that it is no easy matter to quote offhand any suitable example of

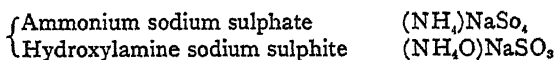
a simple or easily explained character. Probably the neatest case is that afforded by the $\beta\beta$ and $\alpha\beta$ disulphonates of hydroxylamine,



recently described by Haga (*Trans. Chem. Soc.* 1906, 89, 240); simpler examples such as



break down experimentally owing to the instability of the compounds formulated in italics, and it is at least doubtful whether compounds such as



have ever been crystallised out. The derivatives of cobalt, chromium, and platinum have given a number of examples in which compounds of identical composition can be prepared in differently coloured forms, some of the simplest being a pair of chromous chlorides $\text{CrCl}_2 \cdot 4\text{H}_2\text{O}$ (E. M. Rich, *Proc. Chem. Soc.* 1908, 24, 215), pairs of ammoniated cobaltinitrite salts such as $\text{Co}(\text{NO}_2)_2 \cdot \text{Cl}$, 4NH_3 , and pairs of ammoniated platinichlorides and bromides such as $\text{PtCl}_4 \cdot 2\text{NH}_3$; but these can only be explained by somewhat complex space formulæ (see Stewart, *Stereochemistry*, Part II. chaps. v.-vii), and cannot be adequately dealt with in an elementary discussion of the subject.

Isomerism of Organic Compounds. — Subsequent experience has shown that organic compounds present an extraordinarily fertile field for studying the phenomena now under consideration, but the earliest work on the subject presented difficulties of a very high order. Lavoisier's early organic analyses were made with mercuric oxide; those of Gay Lussac were effected by the ignition of pellets in which the organic compound was mixed with potassium chlorate and ignited, the gases produced being analysed volumetrically; those of Berzelius by a similar method, using powdered chlorate in a tube closed at one end.¹

¹ Compare the estimation of nitrogen by Dumas's method as described by Gattermann (*Praxis des organischen Chemikers*, p. 84).

The use of copper oxide was introduced by Gay Lussac in 1815, but was only developed into a perfect method some years later in the hands of Dumas and Liebig. So uncertain, indeed, were the results of organic analysis considered to be that Berzelius in 1819 was actually able to ridicule the idea that organic compounds could exist which contained only carbon and hydrogen, and no oxygen, in spite of the fact that this had been established by the repeated analyses of de'Saussure. It is therefore little wonder that when Liebig, working in Gay Lussac's laboratory, found that silver fulminate had the same percentage composition as the silver cyanate discovered and analysed by Wöhler in 1822, the result was not accepted until the analyses had been repeated and confirmed by both investigators. Even then Berzelius, whose custom it was in his annual reports to sit in judgment on the work of all his contemporary chemists, could not be persuaded to pass a favourable verdict until his own experiments had demonstrated in 1830 that racemic acid—discovered by Kestner (1822) as a by-product in the separation of tartaric acid from argol and investigated by Gay Lussac in 1826—was identical in composition with Scheele's tartaric acid, although showing marked differences in its physical properties. Being thus convinced by his own experience of the reality of the phenomenon, he proposed in his Annual Report of 1831 that substances which thus combine identity of composition with distinct physical and chemical characteristics should be described by the word *Isomeric*.

Polymerism.—Whilst the discussion in reference to the composition of the cyanates and fulminates was still in progress, an equally striking example of a similar kind was forthcoming from the work of Faraday on the hydrocarbons condensed in the manufacture of "portable gas." The discovery of the hydrocarbon benzene in the liquid portion was followed in 1825 by the investigation of a more volatile substance which escaped in the form of vapour when the liquid was removed from the gas-holders. This vapour had the same composition as olefiant gas, and like it combined with its own volume of chlorine; but its density was twice as great, and its chloride contained only half as much chlorine. In this case, it was clear that the two compounds differed in molecular complexity,¹ and in order to avoid confusion Berzelius proposed in 1832 to restrict

¹ Ethylene = C_2H_4 ; butylene = C_4H_8 .

the term "isomeric" to the cases in which no such difference was present, and to describe those substances in which the relative numbers of the atoms were the same, but the absolute numbers unequal, as *Polymeric*.

Metamerism or Isomeric Change.—The same decade which witnessed the discovery of isomerism by Wöhler and Liebig, and of polymerism by Davy, was rendered memorable by a further discovery arising out of Wöhler's work on the cyanates. After investigating a number of the metallic salts of cyanic acid, he attempted to enlarge their number by the preparation of an ammonium salt by the action of ammonia on lead cyanate. A soluble substance was formed, but after evaporating to dryness the product was identified as *urea*. This preparation attracted great attention at the time, and for many years afterwards, as a conversion of mineral matter into a typical organic product: how far this view was legitimate in view of the animal sources ("horns, hoofs, and hides"—to quote the conventional statement of the textbooks) from which the cyanides were invariably prepared, is open to question, but the observation was at least of striking importance as the first known example of *Isomeric Change*.

Substances such as ammonium cyanate and urea which could thus pass over into one another, by *keeping* or by *change of temperature*, without any change of composition or any alteration in the size of the molecule, Berzelius proposed to call *metameric*, using (as he says) the prefix "meta" as in "metamorphosis" to indicate change. As illustrations of this relationship he quoted two examples, one from inorganic and the other from organic chemistry. The former of these—the isomerism of basic stannic sulphite $(\text{SnO})\text{SO}_3$, with stannous sulphate SnSO_4 —appears to be entirely hypothetical. The organic example of cyanic and cyanuric acids, another quotation from Wöhler's fruitful work on the cyanates, has since been shown to be vitiated by an alteration in the size of the molecule, so that the conversion of cyanuric acid by distillation into cyanic acid, and of cyanic acid by keeping into cyamelide, are really cases of depolymerism and of polymerism rather than of metamerism or isomeric change.

It is altogether unfortunate that the word thus excellently chosen and defined by Berzelius should have been diverted from its original meaning and attached to other phenomena—

varying, indeed, in an arbitrary manner with the whim of each individual writer—so that its true significance has been entirely obscured. It is perhaps fortunate that the chaotic usage of the word has, as a natural consequence, rendered it practically obsolete at the present time, since it is likely that after a few years the abuse of the word will so far have been forgotten that it may once again become possible to use it in its original sense.

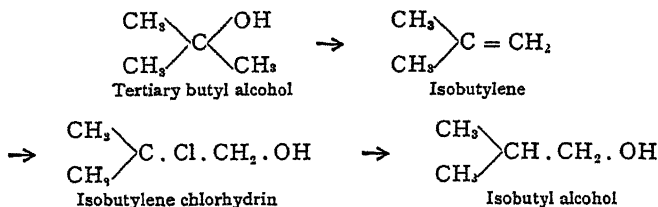
Reversal of Isomeric Change.—Only one further stage in the history of isomeric change need be noticed at the present moment. The distillation of cyanuric acid resulted in its *complete* conversion into cyanic acid, which in its turn passed over entirely into the porcelain-like cyamelide. So, too, when ammonium cyanate was prepared by the action of ammonia on the lead salt, the evaporation of the solution led to the appearance of urea as the sole product of the action. For half a century the impression prevailed that all isomeric changes must be of this type, proceeding to completion in one direction or the other according to the relative stability of the two compounds. It is therefore impossible to speak too highly of the investigations of Butlerow, which proved for the first time that isomeric change might be reversible. Butlerow's observations and the conclusions he drew from them provided a fitting completion of the work begun by Wöhler fifty years before, and although the thirty years that have since elapsed have provided innumerable examples in confirmation of his views, they have, with one possible exception, added nothing to his clear conceptions in reference to the phenomena of reversible isomeric change.

Butlerow's Work on Butylene and Butylalcohol.—Butlerow's discovery was made in the course of a series of experiments on the conversion of the hydrocarbon butylene, C_4H_8 , by the action of sulphuric acid, on the one hand into butyl alcohols of the formula $C_4H_{10}O$, and on the other hand into polymeric olefines such as C_8H_{16} and $C_{12}H_{24}$. This investigation may be regarded as an extension of the work of Faraday, to which reference has already been made, on the relationship between ethylene C_2H^4 and butylene C_4H_8 , and formed part of a general study of the polymerisation of the olefines and their interconversion with the related alcohols.

As early as 1873 Butlerow had noticed the presence of

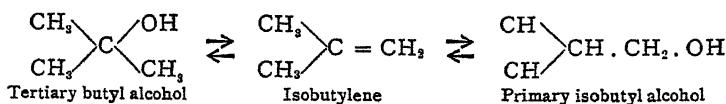
isobutyric acid $\begin{matrix} \text{CH}_3 \\ \text{CH}_3 \end{matrix} \text{>CH} \cdot \text{CO} \cdot \text{OH}$ amongst the products obtained when carefully purified crystalline specimens of trimethyl carbinol, $\text{C}(\text{CH}_3)_3 \cdot \text{OH}$, were oxidised by means of a bichromate and sulphuric acid mixture. The formation of this acid was a clear indication that the liquid contained, in addition to the original tertiary compound, a primary isobutyl alcohol $\begin{matrix} \text{CH}_3 \\ \text{CH}_3 \end{matrix} \text{>CH} \cdot \text{CH}_2 \cdot \text{OH}$.

That the two alcohols were closely related was already evident from observations made six years previously. The tertiary compound had been detected—as an impurity or as a product of change—in commercial isobutyl alcohol after conversion into the chloride (*Annalen*, 1867, 144, 34), and an actual change from one alcohol into the other had been effected (*loc. cit.* p. 30) by converting the tertiary alcohol by the action of sulphuric acid (1 vol. to 2 vols. of water) into isobutylene, combining this with hypochlorous acid and reducing out the chlorine with sodium amalgam—



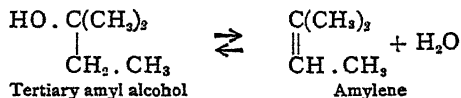
A more direct conversion of the isobutylene into a butyl alcohol, by absorbing it in sulphuric acid (2 vols. to 1 vol. water), diluting and distilling, had led to the reproduction of the original tertiary compound as the main product of the action (*loc. cit.* p. 22), but no method was available for the ready separation of the non-crystalline primary isomeride, and no attempt was therefore made at the time to detect it as a minor constituent of the resulting alcohol, although the possibility of its formation was clearly recognised. When, however, it was found in 1873 that its presence in the products of interaction of tertiary butyl alcohol and sulphuric acid could be demonstrated by its ready oxidation to isobutyric acid, it was at once evident that its formation must be ascribed to a reversible dehydration of the alcohol followed by recombination

with water (or with sulphuric acid) in both of the two possible ways :



Amylene and Amyl Alcohol.—Although this important observation was made in 1873, it was not until four years later that Butlerow was able to make a clear pronouncement on the subject of reversible isomeric change. The paper, "Ueber Isodibutylen" (*Annalen*, 1877, **189**, 44–83), in which his views were first fully developed, will always hold rank as a classic, and can be regarded as second in value only to the papers in which Liebig and Wöhler announced their discoveries of isomerism and of isomeric change half a century earlier.

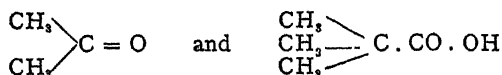
A part of the paper is devoted to observations on the hydrocarbon amylene, C_6H_{10} , a compound intermediate in complexity between isobutylene C_4H_8 and isodibutylene C_8H_{16} . Although very ready to undergo polymerisation, the hydrocarbon could be prepared in a fairly pure form by heating tertiary amyl alcohol in a sealed tube at 100° with two or three volumes of a mixture of equal weights of sulphuric acid and water; under these conditions it separated as an oil on the surface of the acid, but when the mixture was allowed to cool and shaken repeatedly, the oil re-dissolved in the acid (by conversion into the alcohol or its sulphate), leaving only a small residue of diamylene, $\text{C}_{10}\text{H}_{20}$, on the surface—



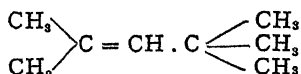
By heating and cooling alternately these changes could be repeated to any desired extent. In this way the reversibility of the hydration and dehydration, postulated in the case of isobutylene, was experimentally demonstrated as a general action which might be assumed to take place to a greater or less extent whenever an alcohol or an olefine was brought into contact with a sulphuric acid of moderate strength.

Isodibutylene.—Isodibutylene, the main subject of the memoir, was prepared by the action of 50 per cent. sulphuric acid on

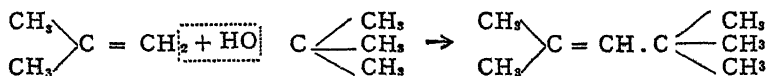
tertiary butyl alcohol. Its main oxidation products were acetone and trimethylacetic acid,



and its constitution was therefore—

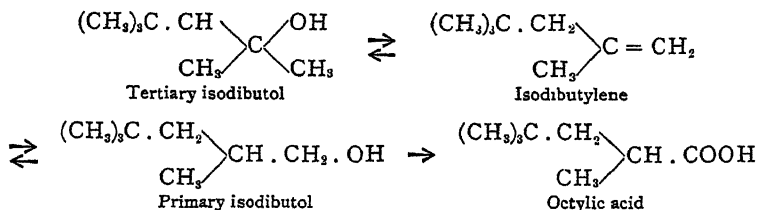


a structure that could be accounted for on the view that the actual condensation took place between a molecule of the tertiary alcohol and a molecule of isobutylene derived from it by the removal of water.



This structure was confirmed by combining the hydrocarbon with hydriodic acid, and hydrolysing the iodide by means of moist silver oxide. The product, "isodibutol," had the composition indicated by the formula $\text{C}_8\text{H}_{18}\text{O}$, and showed all the properties of the tertiary alcohol $\text{HO} \cdot \text{C}(\text{CH}_3)_2 \cdot \text{CH}_2 \cdot \text{C}(\text{CH}_3)_3$, including a marked stability towards oxidising agents. When the oxidation was forced by using a concentrated acid mixture, the products of the action were the unchanged alcohol, isodibutylene—formed by the dehydrating action of the sulphuric acid—and oxidation products derived from it: no direct oxidation of the alcohol could be detected. The experiments served, however, to show that the interconversion of olefine and tertiary alcohol, noticed in the case of butylene and amylene, took place also in the case of isodibutylene, and a more detailed study of the oxidation revealed the presence of an even more complex equilibrium involving not only two isomeric alcohols, but two isomeric olefines.

An oily acid left behind after the distillation of the trimethylacetic acid proved to be an octylic acid of the formula $\text{C}_8\text{H}_{16}\text{O}_2$, and the conditions of its formation left no doubt that it was the analogue of the isobutyric acid obtained as a by-product in the oxidation of tertiary butyl alcohol, and that its formation was due to a similar series of changes as set forth below:



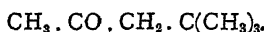
The remaining oxidation product was a ketone, $\text{C}_7\text{H}_{14}\text{O}$, which was oxidised by a stronger chromic acid mixture to acetic acid,



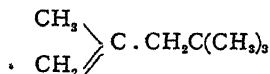
and trimethylacetic acid,



and was therefore formulated as—

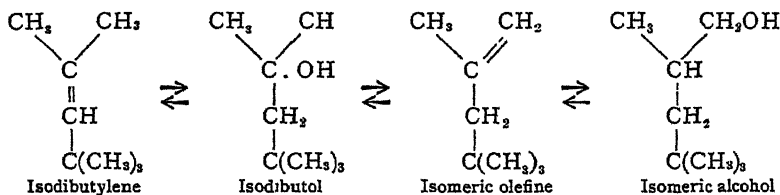


The ketone was evidently produced from the olefine

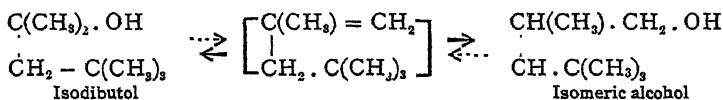


by oxidation of the $=\text{CH}_2$ group, and indicated the presence of this new hydrocarbon in the oxidising mixture.

The complete equilibrium established by means of sulphuric acid acting on isodibutol or on isodibutylene may be represented by the scheme—

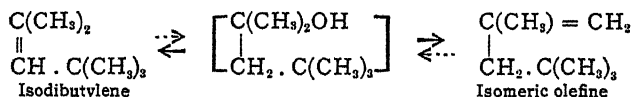


At ordinary temperatures the proportion of hydrocarbon would be very small, and the main action would be an equilibrium between two alcohols through the intermediate olefine,



At high temperatures, on the other hand, the two olefines would

predominate, but these would be brought into equilibrium by the presence of a small amount of the intermediate alcohol,



The complete equilibrium between the four compounds could therefore be reduced by varying the conditions to either of two simple cases of reversible isomeric change.

Isomeric Change without a Catalyst.—It was one of the striking merits of Butlerow's observations that they not only established the existence of reversible isomeric changes, but also revealed the mechanism by which these changes were brought about. In the absence of the acid, or on diluting it with water, the hydration of the olefine and the dehydration of the alcohol were alike suspended. Under these conditions, therefore, dynamic equilibrium between isomers was no longer possible, and the isomerides became as stable as, for instance, ethyl alcohol, $\text{CH}_3 \cdot \text{CH}_2 \cdot \text{OH}$, and methyl ether, $\text{CH}_3 \cdot \text{O} \cdot \text{CH}_3$. As Butlerow expressed it, a special reagent—sulphuric acid—was necessary to bring about in the tertiary alcohols the "*condition of equilibrium, depending on incessant isomeric change*," which he had studied so successfully in the derivatives of isodibutylene.

But is such a special agent always necessary in order to establish a reversible isomeric change? In Butlerow's opinion this was not so, and his discussion of this alternative type of change forms one of the most important features of his paper. Recent work has rendered it doubtful whether such cases actually exist; but a large number of instances are known in which a condition of equilibrium is established in presence of traces of alkaline and saline impurity so minute that it is a matter of the utmost difficulty to get rid of them, and unless extraordinary precautions are taken the state of affairs postulated by Butlerow is substantially realised—at least, to the extent that isomeric change proceeds without the *conscious* addition of a catalytic agent. The conclusions which he drew in reference to compounds which behaved in this way may be described most clearly by a direct paraphrase:

"In such cases, all attempts to establish a definite chemical structure for the substance would be useless, since molecules of the two or more isomeric varieties would always be present. It

is clear that the chemical metamorphoses of such a substance must proceed sometimes in the sense of one chemical grouping, and sometimes in the sense of the other, according to the nature of the reagent and the conditions of the experiment. As an example of such a two-sided chemical structure, one might probably adduce the cases of cyanic acid, hydrocyanic acid, and so forth. From this point of view it appears both unnecessary and impracticable to determine whether cyanic acid is a carbimide or the hydroxide of cyanogen, and whether prussic acid is a nitrite or an isonitrite. . . . It is scarcely necessary to add that the ideas which I have here developed are an application to the principles of chemical structure of the dynamic theory, the foundations of which were laid by Berthollet."

For this condition of *dynamic equilibrium between isomers*, no new name was proposed by Butlerow; but it may conveniently be referred to as *dynamic isomerism*, a name which has the merit of summarising—without adding anything to—the descriptive phrases which have been quoted above.¹

Reversible Isomeric Change as an example of Mass-action.—It is evidence of the essential soundness of Butlerow's judgment that he was content to explain the properties of substances like cyanic and hydrocyanic acids by extending to these more difficult problems the laws that had been established by the study of more commonplace phenomena. For him, the hypothetical equilibrium between cyanic and isocyanic acids was no mysterious or even peculiar action, but a normal type of chemical change differing in no essential feature from a large range of well-established equilibria. His reference to Berthollet shows that he wished to represent the dynamic isomerism of

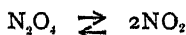
¹ A remark in a recent text-book to the effect that "The Hantzsch and Herrmann view of desmotropy has recently been revived by Lowry in a special case under the name (borrowed from Bischoff) 'dynamic isomerism,'" can scarcely be passed over without comment. The phrase "dynamic isomerism" was in reality not borrowed from Bischoff, but was an adaptation of the adjective "*isodynamic*" which had been proposed by Armstrong in 1889 (Morley and Muir's edition of *Watt's Dictionary*, article "Isomerism") as a substitute for Berzelius's "meta-meric." So far as the theory itself is concerned, a complete statement of its essential features was given by Butlerow in 1877, and it is therefore hardly correct to suggest that in calling attention to his conceptions I was reviving the views advocated by Hantzsch and Herrmann ten years later. The discovery of a phenomenon is necessarily of greater importance than the attachment to it of a new label, or the correction of a subsequent error, and in this respect no later worker is in a position to dispute Butlerow's claim to a unique position in the development of the theory of dynamic isomerism.

the cyanogen compounds as analogous with the reversible actions which Berthollet had studied in aqueous salt-solutions, for instance—

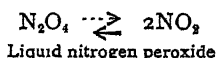


(*Essai de Statique chimique*, 1803, vol. i. p. 99), a case in which by varying the proportions slightly it was possible to crystallise out from the solutions either the chloride or the nitrate of potassium, quite independently of whether the initial materials were calcium chloride and potassium nitrate or calcium nitrate and potassium chloride, and without the addition of any substance comparable with the sulphuric acid used to establish a condition of equilibrium between the isodibutylenes.

In order to elucidate his views, Butlerow also compared the reversible isomeric changes of cyanic and hydrocyanic acids with the conception of "molecular concurrence," which Pfannndler had introduced in a paper on "The Struggle for Existence amongst Molecules: a further contribution to Chemical Statics" (*Pogg. Ann.* Jubelband, 1874, 182), in order to describe the "*simultaneous reciprocal reactions*," which he recognised as occurring in the double decompositions of Berthollet and the dissociation phenomena studied by St. Claire Deville. The comparison of dynamic isomerism with the "simultaneous reciprocal reactions" of dissociation was particularly apt, as may be shown by reference to one of the actual cases investigated by Deville. Nitrogen peroxide, he found, could not be represented by any one single formula, since its vapour density proved it to consist—at all temperatures between 20° and 150°—of a mixture of the dioxide and tetroxide in equilibrium with one another; the gas must therefore be formulated as

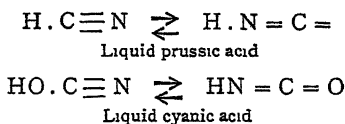


In the case of the liquid this form of evidence is no longer available; but if the colour of the dioxide be used as a test for its presence, the liquid also must be represented by a balanced equation, but showing the tetroxide as the dominant constituent

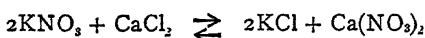


If then, as Butlerow suggests, the spontaneous occurrence of a reversible isomeric change is a cognate phenomenon, hydro-

cyanic and cyanic acids must be represented by equations instead of by simple formulæ, and may be shown as



This simple method of formulation serves admirably to express the essential features of the theory of dynamic isomerism, and brings out with special clearness its analogy with reversible actions of other kinds. Thus, just as the equilibrium



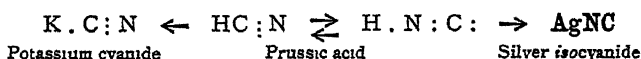
may be carried right over in one direction, by substituting silver for potassium nitrate,



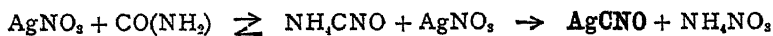
and in the other direction by substituting lead for calcium nitrate,



as a consequence of the sparing solubility of the chlorides of lead and silver, so the equilibrium of prussic acid may be pictured as being overturned in one direction by the action of caustic potash, and in the other direction by the addition of a silver salt,



Again, it is equally easy to express the reconversion of urea into a cyanate in presence of silver nitrate by the symbols



since the change is one which depends mainly on the insolubility of the silver cyanate for the reversal of an equilibrium which normally acts in favour of the formation of urea at the expense of ammonium cyanate.

Crystallisation of Dynamic Isomerides.—It is a noteworthy fact that Butlerow, clinging to past experience as a safe guide in exploring the unknown phenomena of dynamic isomerism, not merely selected two *liquid* acids as types of reversible isomeric change, but deliberately restricted his consideration of equilibrium between isomers to liquids and gases. This limitation,

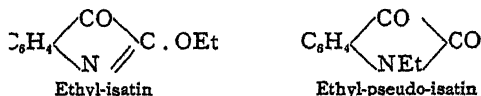
which has been abundantly justified by the observations of later workers, followed as a logical result from the consideration of the analogous cases discussed above. Thus the double decomposition of pairs of salts is usually confined to cases in which at least one of the interacting compounds is in the dissolved or liquid state, and dissociation is equally a characteristic of the fluid and not of the solid condition. Of this, nitrogen peroxide affords an excellent illustration, for whilst the gas is brown, and the liquid straw-yellow, the solid which separates on further cooling forms an absolutely colourless ice, and therefore consists entirely of the tetroxide; similar considerations doubtless apply to the conversion of water into ice, although the evidence for the homogeneity of the solid is less conclusive than in the previous case: in the cases of ammonium chloride and phosphorus pentachloride, however, a direct analytical proof is possible, since the solid that separates has a constant composition even when the vapour contains a variable excess of one of the dissociation-products. The application of those observations to the analogous phenomena of dynamic isomerism is obvious, namely, that whilst oscillatory isomeric changes may occur freely, as Butlerow suggested, in gases and liquids, the separation of crystals will usually result in the production of a homogeneous solid in which no further change can take place.

Arrest of Isomeric Change.—One other important result follows from Butlerow's close association of reversible isomeric change with chemical actions of other kinds, namely, that any development of the theory of chemical change must apply equally to this as to other cases. When, therefore, it became evident from the works of Dixon (*Chem. News*, 1882, 46, 151) that the presence of moisture was an essential factor in the explosive combustion of carbonic oxide, and from the observations of Baker (*Phil. Trans.* 1888, 179, A 571) that similar considerations applied to the combustion of carbon and of phosphorus in oxygen, it became likely that the presence of a third substance or catalyst might be necessary in all cases of chemical change, including those now under consideration. This probability ripened almost into certainty when Baker showed that moisture was necessary to promote both the union of ammonia and hydrogen chloride and the decomposition of ammonium chloride by heat (*Trans. Chem. Soc.* 1894, 65,

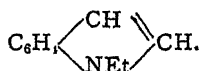
611; 1898, 73, 422); and that by careful drying it was possible also to prevent the dissociation of mercurous chloride (*Trans.* 1900, 77, 646) and of nitrogen trioxide (*Trans.* 1907, 91, 1862). The extraordinary care which Baker used in order to ensure successful results suggested that it might be extremely difficult to repeat in the case of reversible isomeric changes his achievements in preventing dissociation by the removal of impurities. It was, therefore, a matter of special gratification when it was found possible by making use of chloroform as a solvent—without taking any exceptional means to secure the absolute purity of the materials—to arrest the isomeric change of nitrocamphor during a period of two or three weeks (*Trans.* 1899, 75, 219). These isolated observations were subsequently explained by the presence in the chloroform of an impurity (carbonyl chloride) which interacted with the catalyst (ammonia) normally associated with the nitro-compound, in such a way as to destroy its power to initiate chemical change (*Trans. Chem. Soc.* 1908, 93, 119). Up to the present this “arrest” of the oscillatory isomeric change has only been observed in the case of nitrocamphor dissolved in chloroform, benzene, carbon disulphide, or ether, but there can be little doubt that the phenomenon is a general one, and that—contrary to the original suggestion of Butlerow—a “condition of equilibrium depending on incessant isomeric change” is possible only in presence of some third substance possessing definite catalytic properties. In this class may be placed a number of oxygenated solvents, such as water, the alcohols and the organic acids, but when inert hydrocarbons are used, the further addition of moisture, alkali, base or acid is necessary to establish a condition of equilibrium. That Butlerow was not able to foresee this development of his theory is not surprising in view of the fact that the first of the observations quoted above in illustration of the importance of moisture in promoting chemical change was not made until six years after the appearance of his paper. In every other respect his conceptions hold good at the present day, and although erroneous views have frequently been advocated by later writers, the one outstanding feature of the succeeding thirty years’ work has been its complete corroboration of the ideas he expressed so clearly in 1877.

Tautomerism—A False Hypothesis.—The observations discussed in the preceding pages of this article form substantially a complete record of the development of the theory of isomeric change from the earliest times to the present day, and it would scarcely be necessary to add anything further if it were not for the fact that an alternate hypothesis—advanced by Laar in 1885 and almost immediately shown to be false—attracted so much attention and left such a substantial trail behind, that it is hardly possible to conclude without referring both to the theory and to the experimental facts which brought about its downfall.

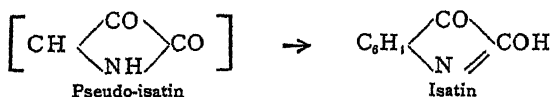
The significance of this theory will be seen most clearly if reference is first made to the observation of Baeyer on isatin and indoxyl (*Ber.* 1883, **16**, 2188). Isatin, like prussic acid, was found to be capable of yielding two isomeric ethyl-derivatives,



the former prepared directly from isatin, the latter by oxidation from ethylindol—



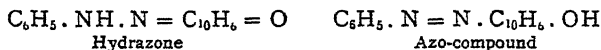
As only one form of the parent substance was known, Baeyer suggested that ethyl-pseudo-isatin was derived from a labile isomeride which reverted to isatin whenever attempts were made to prepare it—



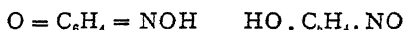
This suggestion differed from the explanation which Butlerow had given in the case of prussic acid by denying the existence of reverse action both in the liquid and in the solid state; but it was evidently only a limiting case, such as Butlerow had himself foreseen, and was in no way incompatible with the views he had advocated.

Laar's paper, "Ueber die Möglichkeit mehrerer Strukturformeln für dieselbe chemische Verbindung" (*Ber.* 1885, **18**,

648), was based on an observation by Zincke (*Ber.* 1884, 17, 3030) which had shown that the phenyl hydrazone derived from naphthoquinone was identical with the phenylazo-derivative of α -naphthol, with which it should have been isomeric.



These observations, as well as the analogous experiments which had shown the identity of quinone monoxime and nitrosophenol,



might have been explained either by Butlerow's theory, or by the theory of labile isomerism made use of by Baeyer, but Laar preferred to develop a new hypothesis, derived in part from the benzene-theory of Kekule. It will be remembered that in order to account for the identity of the 1:2 and 1:6 di-derivatives of benzene, Kekule was led to advocate a kinetic conception of atomic linkages according to which a double bond merely represented a double number of collisions in unit time between the two atoms so united (*Ann.* 1872, 162, 86); as a result of this view it followed that the single and double bonds in a benzene molecule could be represented as occupying the 1:2 or the 1:6 positions indifferently, according to the interval over which the collisions were counted. This loose view of the nature of atomic linkages was boldly transferred by Laar to the whole range of cases in which attempts to prepare isomeric substances had given rise to identical products. Many of these involved the transference of a hydrogen atom in addition to a shifting of the bonds, and it was therefore necessary to assume that the atoms as well as their linkages were in a constant state of flux. Compounds which in consequence of this incessant motion of bonds or atoms could never be properly represented by a single formula, Laar proposed to call "tautomeric."

It was apparently Butlerow himself who called Laar's attention to the alternative explanation which he had suggested eight years previously, and so elicited from Laar a further explanation of his hypothesis ("Ueber die Hypothese der wechselnden Bindung," *Ber.* 1886, 19, 730), in which special attention was directed to points of contrast. These proved to be of fundamental importance. Butlerow had postulated the existence

of pairs of compounds undergoing reciprocal isomeric change with greater or less velocity, according to the ordinary laws of chemical action; Laar substituted for this the idea of an incessant wobble taking place entirely within the molecule, uncontrolled by any of the ordinary chemical laws, and (if the expression can be permitted) physical rather than chemical in character. This point he made clear by repudiating the analogy of dissociation, and substituting for it Maxwell's idea of molecular vibrations giving rise to the production of spectra. In Laar's opinion, then, quinone monoxime and nitrosophenol, or the two forms of prussic acid, were merely *phases* in the motion of a vibrating molecule, and the attempt to prepare either of them separately was as hopeless as if one were to try and collect sodium atoms in the different attitudes involved in the emission of the D_1 and D_2 lines.

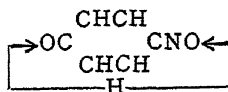
Does Tautomerism Exist?—Laar's ingenious and highly speculative theory attracted a considerable amount of notice, and served a useful purpose in calling attention to a large number of cases in which isomeric compounds were either not capable of separate existence or were converted into one another with extreme readiness; but it was not by any means generally accepted, and was not destined to survive for long the ordeal of experimental verification. Jacobson (*Ber.* 1887, 20, 1732 footnote; 1888, 21, 2628 footnote) was one of the first to repudiate both the theory and the name by which it was described.

The word "tautomerism" is based on Laar's view, which (I believe) is not shared by most chemists, that the molecules of compounds whose chemical behaviour is represented by two structural formulæ differing in the point of attachment of a hydrogen atom, never assume a definite constitution, but exist in a constant state of oscillatory change. The majority of chemists would explain the observations in question in this way, that the known forms of such compounds are to be represented by a definite grouping of atoms which in certain reactions passes over into an isomeric grouping by a rearrangement of bonds consequent upon the displacement of a hydrogen atom.

This process Jacobson proposed to call "*desmotropy*" or "bond-shifting"; the term "*merotropy*" has also been used to express the same idea (see, for instance, Michael, "Ueber Desmotropie und Merotropie," *Ann.*, 1908, 363, 20 *et seq.*), leav-

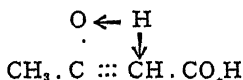
ing Laar to demonstrate, if he could, the concrete reality of his hypothesis of "tautomerism." In view of the close resemblance between this theory and Baeyer's conception of "pseudomerism,"¹ it is open to question whether an additional name was really necessary, but the explanatory statement quoted above is broad enough to include all the cases of dynamic isomerism which involve the migration of hydrogen atoms, and if the new label had remained undisfigured it might have provided a useful title by which these phenomena could be selectively described. Unfortunately, Jacobson's suggestion was immediately followed by one of those foolish attempts to attach a new meaning to an old word which inevitably lead to confusion, owing to the fact that whilst some are content to read the gloss, others will continue to decipher the original text of the palimpsest. Hantzsch and Herrmann (*Ber.* 1887, 20, 2802), although rejecting Laar's theory, proposed to annex the title he had devised for it, and to use it as a general description for compounds which could react in two or more different ways; at the same time they proposed to alter the meaning of Jacobson's word by extending it to isomeric changes not involving the transference of hydrogen but limiting it to those cases in which isomeric forms could be crystallised out—a matter that is entirely dependent on the accident of success or failure in experimental work and has no theoretical significance whatever. The unfortunate result of these suggestions has been that no definite meaning now attaches to either word unless accompanied by some further statement of the sense in which it is used. The only safe rule in all such cases is to adhere strictly to the original significance, and therefore, in opposition to the lax usage now in vogue, to confine the use of Laar's word to the phenomenon of "tautomerism" in the sense in which he understood it.

Does tautomerism, then, exist? Most of the cases that Laar quoted as illustrations involved the oscillation of a hydrogen atom between oxygen and oxygen as in quinone monoxime or nitrosophenol:

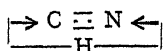


¹ The name is due to Laar (*Ber.* 1886, 19, 730).

between carbon and oxygen as in acetoacetic or oxycrotonic acid :

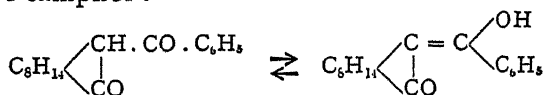


or between carbon and nitrogen as in prussic acid :



The first of these substances has been prepared in a green and in a colourless form to which distinct formulæ must obviously be assigned ; the second substance does not present favourable material for investigation, but a large number of its derivatives have been separated in isomeric forms, one of them, ethyl diacetylsuccinate (Knorr, *Annalen*, 1899, **306**, 332-393), being known in no less than five ; the third case still awaits detailed study.¹

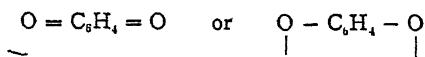
The isolation of labile isomerides in well-defined crystalline forms has been so frequently observed in recent years as to dispose once and for all of the idea that they can be regarded as merely "phases" in the motion of a vibrating material. Confirmatory evidence is supplied by the relative slowness with which these substances change into one another, as is proved by the fact that the isomeric forms of many typical diketones, such as α -benzoyl-camphor :



(Forster, *Trans.* 1901, **79**, 987), and the substances investigated by Knorr (*loc. cit.*), can be separately recrystallised if care is taken to carry out the operation quickly and at a moderate temperature. The most conclusive evidence that the changes in question are intermolecular and not intramolecular (a point which Laar himself selected as illustrating the contrast between his views and those of Butlerow) is, however, that which is afforded by the necessity for a catalyst to establish a condition of equilibrium between the various forms: a change which thus requires the co-operation of a system of molecules must obviously be of the former and not of the latter class.

¹ Michael and Hibbert (*Ann.*, 1909, **364**, 64-76) have during the present year put forward evidence to show that prussic acid possesses a nitrite structure, $\text{H} \cdot \text{C} \vdots \text{N}$.

It is abundantly clear from these observations that the main applications of Laar's theory have proved to be incorrect when examined in the light of subsequent experience. A secondary application, referred to incidentally in his earlier paper (p. 656), remains to be dealt with. The only case cited is that of quinone, which may be formulated as



and, according to the theory of tautomerism, should be in a constant state of vibration or oscillation between these two forms. The benzene compounds discussed by Kekule form another instance in which a constant motion of the bonds is postulated, although the atoms are supposed to retain the same relative arrangement throughout. Laar's suggestion has recently been revived by Baly (*Trans.* 1906, 89, 498) under the name of "isorropesis" in connection with certain optical phenomena which it is proposed to discuss in a subsequent article. At present, therefore, it will suffice to say that the existence of isomeric forms of quinone is a matter of pure speculation, unsupported by any concrete evidence whatever, and that, until some such support is forthcoming, the whole conception of "tautomerism" must be regarded as an ingenious and fascinating hypothesis, but nevertheless mere fiction.

THE THIRST OF SALTED WATER OR THE IONS OVERBOARD

By HENRY ARMSTRONG, PH.D., LL.D., F.R.S.

"A beautiful theory spoilt by a nasty, ugly little fact."—HUXLEY.¹

"It is as risky for a chemist to apply mathematics as for a mathematician to lecture to chemists: we should work in co-operation. . . . All this hangs together but it lends no support at all to the dynamically impossible theory that the ions are free."

FITZGERALD, Helmholtz Memorial Lecture.

THE impassioned eloquence and unrivalled power of appreciating natural beauty characteristic of the author of *Modern Painters* are nowhere more apparent than in the opening lines of the section "Of Truth of Water," in that work:

Of all organic substances, acting in their own proper nature and without assistance or combination, water is the most wonderful. If we think of it as the source of all the changefulness and beauty which we have seen in clouds; then as the instrument by which the earth we have contemplated was modelled into symmetry and its crags chiselled into grace; then as in the form of snow it robes the mountains it has made with that transcendent light which we could not have conceived if we had not seen; then as it exists in the foam of the torrent—in the iris which spans it, in the morning mist which rises from it, in the deep crystalline pools which mirror its hanging shore, in the broad lake and glancing river; finally, in that which is to all human minds the best emblem of unwearied, unconquerable power—the wild, various, fantastic, tameless unity of the sea: what shall we compare to this mighty, this universal element, for glory and for beauty or how shall we follow its eternal changefulness of feeling? It is like trying to paint a soul.

If the painter who is called upon to deal with its surface appearance alone cannot picture water, how shall the chemist succeed in penetrating the hidden mysteries of its being? And yet, as it is his mission to display the soul in all things, the attempt must be made: a careful study of Ruskin's idealism

¹ Vide *Memories of my Life* (Francis Galton).

may be of more service in instructing us how to proceed than any text-book: the counsel he gives may well be considered:

There is hardly a roadside pond or pool which has not as much landscape in it as above it. It is not the brown, muddy, dull thing we suppose it to be; it has a heart like ourselves and in the bottom of that there are the boughs of the tall trees and the blades of the shaking grass and all manner of hues of variable, pleasant light out of the sky; nay, the ugly gutter that stagnates over the drain bars, in the heart of the foul city, is not altogether base; down in that, if you will look deep enough, you may see the dark, serious blue of far-off sky and the passing of pure clouds. It is at your will that you see in that despised stream either the refuse of the street or the image of the sky—so it is with almost all other things that we unkindly despise. Now this far-seeing is just the difference between the great and the vulgar painter; the common man *knows* the roadside pool is muddy and draws its mud; the great painter sees beneath and behind the brown surface what will take him a day's work to follow but he follows it, cost what it will. And if painters would only go out to the nearest common and take the nearest dirty pool among the furze and draw that thoroughly, not considering that it is water that they are drawing and that water must be done in a certain way but drawing determinedly what they *see*, that is to say, all the trees and their shaking leaves and all the hazy passages of disturbing sunshine; and the bottom seen in the clearer little bits at the edge and the stones of it and all the sky and the clouds far down in the middle, drawn as completely and more delicately they must be than the real clouds above, they would come home with such a notion of water-painting as might save me and every one else all trouble of writing more about the matter; but now they do nothing of the kind but take the ugly, round, yellow surface for granted or else improve it and, instead of giving that refined, complex, delicate but saddened and gloomy reflection in the polluted water, they clear it up with coarse flashes of yellow and green and blue and spoil their own eyes and hurt ours, and the uninterrupted streams and maligned sea hiss shame upon us from all their rocky beds and hollow shores.

Have not we chemists done little more thus far than spoil our own eyes and shut those of others for all appreciation of water—have we not made it of little account by representing it by the thin and unattractive symbol H_2O —still more by *speaking* of it with heartless vulgarity in these latter degenerate days as *Aitch-too-oh*? Surely “the uninterrupted streams and the maligned sea hiss shame” upon our science, deriding our

puny efforts to explain the maddening thirst begotten in water by the salts which the unconquerable and untiring scavenger, throughout the ages, has washed from the surface of our globe. While a Coleridge sings :

Water, water, everywhere,
Nor any drop to drink,

the fish, naked but unashamed, in very scorn of our weakness, swim calmly in the ocean and are not harmed. It is only too true that familiarity breeds contempt: we have so lost ourselves in muddy pools; like the common painter we have so fixed our attention on the mud, that we know nought of clean water—clear, persistent thinking alone can give us knowledge; and that has been set aside of late.

Since 1885, my voice has been that of one crying in the wilderness when, time after time, I have insisted that it is essential to take the water in aqueous solutions into account—that water is altogether peculiar among solvents; the meagre answer of those who now are compelled to admit that it is active has been, "It has high specific inductive capacity"—truly a stone for bread. Apparently, the situation has been so trying to the nerves of those afflicted with ionomania that, like the burglarious railway porter in Mr. Barrie's recent play, they have elected to take the solute neat: it might be supposed because the advocates of the doctrine came from colder or damper climes than our own, where water is at a discount, were it not that, while thinking of the cordial, with strange inconsistency they have elected to deal only with beverages containing the merest dash of spirit.

But, as did Sister Anne in the fairy story, I see signs of approaching help: there is reason to think that Wotan's spear is broken at last; that the *Götterdämmerung* must soon be enacted. Thus two writers in a recent number of Liebig's *Annalen der Chemie*, Messrs. Decker and von Fellenberg, not only put forward the original conclusion that oxygen is to be treated as a tetradic element but, such is the encouragement given by recent revolt against imperial autocracy—actually dare to murmur against ionic autocracy. Their words are noteworthy:

In den verdünnten Lösungen von Salzsäure in Wasser ist Oxoniumchlorid anzunehmen, genau wie in den Lösungen von Salzsäure in flüssigem Ammoniak Salmiak angenommen wird

und entgegen den Auffassungen der Ionentheorie [my italics]. Solange die letztere die Frage nach der chemischen Rolle des Lösungsmittels stillschweigend umgeht, müssen ihre Ansprüche chemische Thatsachen zu erklären als ungerechtfertigt angesehen werden. Die Anzahl der offenbar zusammenhängenden Thatsachen aber, die die Ionentheorie mit Hülfe der von Faraday als glänzendes Mittel zur Veranschaulichung der Vorgänge bei der Elektrolyse geschaffenen Vorstellung der Ionen zusammenzufassen sucht, wird, wie es scheint, in der wechselnden Valenz bestimmter Atome des Lösungsmittels ihre Erklärung finden. Hierin liegt auch die grosse Bedeutung der Lehre von der Tetravalenz des Sauerstoffs. Sie ist berufen, die structurtheoretische Unterlage für die von Mendelejeff inaugurierte Hydrattheorie der wässerigen Lösungen und für das damit zusammenhängende Problem der Structur des flüssigen Wassers zu liefern.

All this of course without reference to the fact that they are recapitulating ancient history. But what matters it who spreads the complaint? The virus is abroad at last.

At all events the opsonic index of workers is now risen sufficiently high, it seems, to warrant the introduction of a strong dose of antitoxin. Thus another writer, who dates from Charlottenburg—that name of strange import in London—I. Traube, who also apparently is fallen a victim to *Wassersucht*, with strange effrontery actually concludes an essay (*Berichte der deutschen chemischen Gesellschaft*, 1909, p. 94) with the impious words, “Die Theorie von Arrhenius scheint mir auf einen fundamentalen Irrthume zu beruhen.” When, in very early days, I ventured to say that the speculation was an offence against chemical common-sense, I was looked at askance as an infidel. For some time past, Prof. Harry C. Jones, of Baltimore, who has long been a notable trumpeter of ionic formularies in the United States, has been gradually executing strategic movements which give clear indication of a desire to beat a retreat from an impossible position. He is even beginning to claim to have been the inventor of the weapons in the hands of the opposition party—always an interesting sign of recantation. The beautiful simplicity of this writer’s belief is well shown in the following passage from the preface to his *Elements of Organic Chemistry* (1903):

Take the theory of electrolytic dissociation, which to-day is as well established as many of our laws of nature [this is not

saying much for the certainty of the said laws, perhaps]: it has shown us that it is the ions and not the atoms which are the active agents chemically. If the student is taught the contrary in the early stages of his work, later this must all be unlearned and we know how difficult it is to correct first impressions. . . . Further, since we know that the ion and not the atom or the molecule is the factor which enters into most chemical reactions, we should insist upon it because it is true.

The situation is painted in an equally philosophical manner in the preface to his larger work on the *Principles of Inorganic Chemistry*. It is rendered almost amusing by a statement such as the following:

The writer has refrained from introducing unproved theories and disputed questions as far as possible, since the student for whom this work is meant is scarcely at a stage to properly appreciate and evaluate scientific discussion. The attempt has, however, been made to avoid dogmatism, since this is harmful even in an elementary work.

O wad some Pow'r the giftie gie us
To see oursels as ithers see us !

But this may be said of most of us.

Prof. Jones may have reason during the next few years to appreciate the force of his own remark, "we know how difficult it is to correct first impressions"—in any case, the example may serve to show how necessary it is to have regard for facts and to avoid creating visionary impressions.

In our own country, the physico-chemical dovecot has evidently been somewhat disturbed of late: seemingly its inmates are beginning to study the gentle art of hedging; unfortunately most of them lack the courage to release their hold of the Ostwaldian petticoats, to which they have so long been accustomed to cling that they are unable to stand alone. But, as was said of the lambs of little Bo-Peep—if let alone for a while longer, doubtless they will come home and leave their tales (of ions as well as of repentance) discreetly behind them.

It is time that some serious protest were made against the pedantic narrowness and dogmatism of the modern elementary text-book. If text-books were mere novels—they resemble them most closely in the entire absence of novelty of theme—it would matter little what is written in them; unfortunately the poor devil of a student is forced by a merciless examination system to

commit their contents to memory and touching pitch he cannot avoid defilement. The retirement of the censor of stage plays would probably be welcomed by playwrights ; if he were charged instead with the suppression of indecent scientific literature—indecent through lack of logic and lack of most things worth knowing—he might render a real service to society.

After all, we scientific workers (or should it not rather be said we workers in science ? because, although evil communications corrupt good manners, the work of science has not, as a necessary consequence, the establishment in the worker of a scientific habit of mind), like women, are the victims of fashion : at one time we wear dissociated ions, at another electrons ; and we are always loth to don rational clothing ; some fixed belief we must have manufactured for us : we are high or low church, of this or that degree of nonconformity, according to the school in which we are brought up—but the agnostic is always rare among us and of late years the critic has been taboo. The poor student is usually the sore afflicted victim of our whims. Instead of setting him to read sound literature, such as the *Life of Faraday* by Bence Jones and even Faraday's researches, so that he might appreciate the spirit in which work has been done in the past, we administer the latest tips in mild doses : the poor creature's mental digestion is horribly upset and his morals remain lax—but what matters it ? he can use stilted paraphrase in place of the studied simplicity of older times and is up to date, although perhaps he has no knowledge of fundamentals. Our modern methods of teaching are pretentious and too often fraudulent. We need to realise this and to change our ways.

When alcohol is mixed with water what really happens ? When sugar or salt is dissolved, what is the effect produced ? In all cases, whatever is dissolved in it, the properties of the solvent water are profoundly modified—its freezing point being lowered but its boiling point raised, although in different degrees when the substances are used in proportions corresponding to their formulæ, C_2H_6O , $C_{12}H_{22}O_{11}$, $NaCl$, the effect produced in weak solutions by the salt being about 1.75 times as great as that produced by either alcohol or sugar. The solution of salt, moreover, differs from the solutions of the two other substances in that it is an electrolyte—capable, that is to say, of conducting an electric current.

As long ago as 1851, it was argued by Williamson in his classical essay on the *Theory of Electrification*, that—

The formation of ether from alcohol and sulphuric acid is neither a process of simple separation nor one of mere synthesis; but it consists in the substitution of one molecule for another and is effected by double decomposition between two compounds. This view of the matter (he said) is therefore consistent with the contact theory, inasmuch as it acknowledges the circumstance of contact as a necessary condition of the reaction of the molecules upon one another. . . . It may naturally be asked, how do hydrogen and carburetted hydrogen thus continually change places? It cannot be from any such circumstance as superior affinity of one molecule over another, for one moment sees reversed with a new molecule the transfer effected during the preceding one. Now, in reflecting upon this remarkable fact, it strikes the mind at once that the facility of interchange must be greater the more close the analogy between the molecules exchanged; that if hydrogen and amyl can replace one another in a compound, hydrogen and ethyl, which are more nearly allied in composition and properties, must be able to replace one another more easily in the same compound; and that the facility for interchange of hydrogen and methyl, which are still more similar, will be still greater. But if this be true, must not the exchange of one molecule for another of *identical* properties be the most easily effected of all? Surely it must, if there be any difference at all; and if so, the law of analogy forbids our imagining the fact to be peculiar to hydrogen among substances resembling it in other respects. We are thus forced to admit that, in an aggregate of molecules of any compound, there is an exchange constantly going on between the elements which are contained in it. For instance, a drop of hydrochloric acid being supposed to be made up of a great number of molecules of the composition ClH , the proposition at which we have just arrived would lead us to believe that each atom of hydrogen does not remain quietly in juxtaposition with the atom of chlorine with which it first united but, on the contrary, is constantly changing places with other atoms of hydrogen or, what is the same thing, changing chlorine.

It is clear that Williamson thought of the decomposition as taking place during the times when the molecules were in contact. A few years later, in 1857, however, in order to account for the fact that the smallest currents gave rise to a sensible amount of electrolytic decomposition, Clausius introduced the conception of ionic dissociation—of the separation of the compound in solution into its two ions—which he

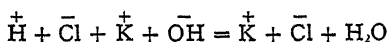
regarded as conditioned by occasional violent kinetic collisions between the molecules. But his point of view was that from which the classical baby in Marryat's novel was apologised for—as being only a little one. It was not until 1883 that Arrhenius came forward as a Whole-Hogger and, in order to account for the increase in the molecular conductivity on diluting the solution of a dissolved salt, assumed that the dissociation might be complete or nearly so. His conception did not become known until late in 1885 and even then did not “catch on.” The way had to be prepared and salted, as chemists were still possessed of some sanity of judgment.

In 1885, Van't Hoff, assuming the character of the Hatter, invited us to a scientific Mad Tea Party, at which he out-hatted the Hatter by gravely assuring us that “I see what I eat” *was* the same thing as “I eat what I see”: introducing us into topsyturvydom, he insisted that a liquid was to be treated not as a liquid but as a gas; that the attractive forces in play between the molecules of solute and solvent were not really attractions: the solute was to be thought of as banging about and hitting things, as though it were gasified. Even this did not mislead us, mainly because, although, in the case of cane sugar, Pfeffer's pressure values—when looked at upside-down—were in accordance with the gas pressure hypothesis, salt again behaved in an irrational way, producing nearly double the effect of sugar.

But here Arrhenius got his chance and being a good sportsman he soon took it, coming forward with the suggestion that the number of molecules in the solution of salt was nearly twice as great as had been supposed, owing to the dissociation of the salt into its ions. The combination of Van't Hoff's great reputation as the author of the asymmetric carbon hypothesis with Arrhenius's juvenile enthusiasm was irresistible: with the aid of floods of Ostwaldian ink, the joke soon spread far outside. The mistake has been made in our own time, therefore we must admit the possibility of making it; but it is none the less difficult to understand how a man of Van't Hoff's genius could possibly have been misled by the observation of a parallelism into the assumption of any actual resemblance between gaseous pressure and the peculiar conditions of stress in a solution.

Both in my address in 1885 to the Chemical Section of the British Association and in my communication to the Royal

Society in the following year (written before I had been able to procure Arrhenius's paper) I took definite exception to the ionic dissociation hypothesis, even in the mild form postulated by Clausius. I also took exception, in the most positive manner possible, to the Arrhenius hypothesis in several of the reports published between 1885 and 1890 by the committee appointed to consider Electrolysis in its Physical and Chemical Bearings. At Leeds, again, in the discussion on the theory of solution, I protested against the acceptance of the ionic equation



in the following terms :

Such a conclusion, although undoubtedly necessary and logical from the dissociationist's point of view, involved the admission that hydrogen chloride and water were compounds of a totally different order; that these two hydrides were so different that while that of chlorine underwent practically complete dissociation, that of oxygen remained practically unchanged. Chemists, however, were in the habit of teaching that chlorine and oxygen were comparable elements and the facts of chemistry appeared to afford the strongest evidence that hydrogen chloride and oxide were in all ways comparable compounds. Moreover, the behaviour of the two compounds at high temperatures afforded no grounds for any such belief in the instability of the one and the stability of the other."

To this Ostwald made reply as follows :

Professor Armstrong has declared that the dissociation theory of electrolytes is unacceptable to chemists. As far as I am aware, there exists nowhere a real contradiction between chemical *facts* and the dissociation theory but this theory only runs against all the time-honoured *feelings* of chemists. As feelings, although very powerful things, are at least variable with time and custom, it is to be expected that they will change sooner or later. The time is not very long past when the assumption that, in the vapour of ammonium chloride, hydrochloric acid and ammonia, which have "so great an affinity for each other," should exist separately from one another, ran in quite the same manner against the feelings of chemists. Now we are accustomed to this conception and in the same manner in a year or two chemists will speak as quietly of the free ions as they now speak of the uncombined mixture of hydrochloric acid and ammonia in the gaseous state. . . .

Professor Armstrong has asked why water does not split into ions, while hydrogen chloride, a body similar to water,

does. But has Professor Armstrong forgotten that liquid hydrogen chloride, like pure water, is an *insulator* for the electric current, as was found long ago by Gore, an observation afterwards confirmed by Bleekrode? It has been stated by F. Kohlrausch that at ordinary temperatures no pure liquid is a good electrolyte. The theory of Arrhenius is still in this point the only one which explains this strange fact; *pure liquids do not conduct, because their molecules have no space to resolve themselves into ions* [my italics].

It is therefore not improbable that water would conduct electrolytically if we could find a suitable solvent for it. An investigation in this direction would be of very great interest but not without grave difficulties.

In a postscript to the German edition of the Report of the Leeds discussion (*Zeits. phys. Chem.* 1891, 7, 418), Ostwald further contended that much greater differences than are apparent between hydrogen chloride and water are noticeable between compounds such as NCl_3 and PCl_3 , N_2O_3 and P_2O_5 , which are more closely related than are hydrogen chloride and oxide.

It has always appeared to me that, in quoting such examples, Ostwald made a particularly unhappy choice, as nitrogen chloride and nitric anhydride are extraordinarily unstable, whilst hydrogen chloride and oxide are *both* eminently stable. His reference to liquid hydrogen chloride being, like water, an insulator, was also inconsequent. Nothing could show more clearly how little chemical feeling he had at that time and how unable he was to read the *facts* of chemistry. Moreover, to cite the dissociation of ammonium chloride as in any way confirmatory of the view that hydrogen chloride is dissociated into its ions in aqueous solution is entirely unjustifiable, especially as ammonia is supposed to remain unaffected: the two cases offer no points of analogy. Twenty years ago, it was clear that a sharp distinction must be drawn between the valency of nitrogen in the ammonium compounds and its valency in ammonia—between pentad and triad nitrogen; all that has happened in the interval serves to confirm this view, especially the considerations advanced by Barlow and Pope in their several memoirs on the correlation of crystalline form with structure.

It is a striking fact that Helmholtz, notwithstanding his partiality to atomic charges of electricity, was disturbed by the liberties taken by the dissociationists. This is clear from the

following passage in his life by Kœnigsberger (English translation, p. 340):

“Nernst has thrown himself zealously into the newest applications of physical chemistry, as worked out by the Dutchman Van't Hoff and advocated with great vigour by Professor Ostwald of Leipzig in his *Journal*. These theories have already proved to be of great practical utility and have led to a multitude of demonstrably correct conclusions, although they imply some arbitrary assumptions which do not seem to me to be proven. The chemists, however, make use of this hypothesis [of the dissociation of a portion of the compound molecules of the dissolved salts] in order to form a clear conception of the processes and they must be allowed to do this after their fashion, since the whole extraordinary comprehensive system of organic chemistry has developed in the most irrational manner, always linked with sensory images, which could not possibly be legitimate in the form in which they are represented. There is a sound core in this whole movement, the application of thermodynamics to chemistry, which is much purer in Planck's work. But thermodynamic laws in their abstract form can only be grasped by rigidly trained mathematicians and are accordingly scarcely accessible to the people who want to do experiments on solutions and their vapour tensions, freezing points, heats of solution, etc.”

The fact which Helmholtz did not sufficiently appreciate was that the men who were taking the liberties he deprecated were not chemists, at least in feeling—that they were men who had thrown chemistry to the winds and were proceeding on hypothetical let-it-be-granted principles. The physico-chemical school, in fact, has never been a school of chemists.

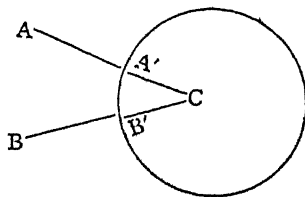
The one chief “nasty, ugly little fact” which has spoilt the dissociation hypothesis is the fact that the solvent has always been neglected by the advocates of the speculation. To Kohlrausch it was simply the screen serving to keep the ions apart. Ostwald at first preached a similar doctrine, arguing—as in the above quotation—that it gave space in which the dissociation could take place. In 1885 and again in 1886, as well as on several later occasions, I took exception to this view and insisted that solvent and solute were reciprocally affected. Gradually this was realised by the dissociationists but they quietly hedged and never had the grace to admit that ionic dissociation was

not of the order of gaseous dissociation—that the behaviour of hydrogen chloride was not that of ammonium chloride.

After 1893 my interpretation was paraphrased, in consequence of a suggestion made by J. J. Thomson and Nernst independently, that the cause of dissociation was the high specific inductive capacity of water. The attempt was then made to show that substances generally of high specific inductive capacity were active in promoting dissociation—but this turned out to be a signal failure in the main. The argument used by J. J. Thomson is of considerable interest (*Phil. Mag.* 1893, **36**, 320):

The view that chemical action is electrolytic in character has been repeatedly urged by Prof. Armstrong. The ability of water to further this kind of action would be much greater if, while the chemical action were going on, the water existed in the liquid than it would if the water were in the gaseous state. If we take the view that the forces which hold the atom in the molecule together are electrical in their origin, it is evident that these forces will be very much diminished when the molecule is close to the surface of or surrounded by a conductor or a substance like water possessing a very large specific inductive capacity.

Then let AB represent two atoms in a molecule placed near a conducting sphere; then the effect of the electricity induced in the sphere by A will be represented by an opposite charge placed at A' the image of A in the sphere. If A is very near the



surface of the sphere, then the negative charge at A' will be very nearly equal to that at A. Thus the effect of the sphere will be practically to neutralise the electric effects of A; as but one of these effects is to hold the atom B in combination, the affinity between the atoms A and B will be almost annulled by the presence of the sphere. Molecules condensed on the surface of a sphere will thus be practically dissociated. The same effect would be produced if the molecules were surrounded by a substance possessing a very large specific inductive capacity. Since water is such a substance, it follows, if we accept the view that the forces between the atoms are electrical in their origin, that when the molecules of a substance are in aqueous solution

the forces between them are very much less than when the molecule is free and in the gaseous state.

The fallacy which underlies the application of this argument does not appear to have been recognised. It has not been sufficiently taken into account that the resolution of the compound A B into A and B under the influence of the water sphere is temporary only, in the sense that it obtains only while the two are in approximation. Their separation is apparent rather than real; in fact, the dissociation is merely potential and there would seem to be no reason why the superior osmotic effects which, *ex hypothesi*, are due to the *free* ions alone should be produced by them in the new state of restraint to which they are subject under the inductive influence.

Apart from the objection that the conception of ionic dissociation is opposed to reason and chemical common-sense, there are other specific objections which appear to render the hypothesis untenable. "L'activité chimique se confonde avec l'activité électrolytique" is the expression used by Arrhenius in 1883; it has misled his followers to the present day. It is true only as regards the order of activity in a series of acids, for example; it is the reverse of true when the comparison is made for various degrees of dilution: it then appears that whilst molecular conductivity increases on dilution, the very opposite is true of hydrolytic activity. The case is one like that propounded by the Duchess when she said, "The more there is of mine, the less there is of yours." If free ions increase in number as a solution is diluted and condition an increased conductivity, if these same ions condition hydrolytic activity, then hydrolytic activity must be directly proportional to the number of free ions. The argument is the same as that favoured by the thermodynamic school, when they confound you by proving that change of a certain character would involve perpetual motion—which is impossible.

The rigidly selective action of enzymes as hydrolytic catalysts is in no way explicable by the ionic dissociation hypothesis—hydrogen ions, which, *ex hypothesi*, are the active agents in hydrolysis, cannot be supposed to act selectively.

Nor can the action of acids as hydrolytic catalysts be explained except from an association standpoint, as when used

in very small amount they simulate absolutely the action of enzymes. At first the action follows a straight-line law, which is proof that the ions are not evenly distributed in the solution, as they should be according to the dissociation hypothesis.

Another and the most forcible of all arguments perhaps against the hypothesis is the proof which has been given, in case after case, by Brereton Baker and others, that the occurrence of chemical change is dependent not on dissociation but on the association in one conducting system of at least three components, one of which is an electrolyte.

Another argument is the preposterous limitation which the hypothesis imposes on our ability to explain chemical change, as it is only applicable to electrolytes, not to the vast host of organic compounds excepting organic acids and alkalis.

But what has weighed more than almost any other consideration with me has always been the absolute and uncompromising attitude of objection to the hypothesis taken by Fitzgerald at British Association meetings and especially in his Helmholtz Memorial Lecture. He alone appeared to me always to understand the situation and to appreciate the difficulties.

The conceptions developed by Van't Hoff, Arrhenius and Ostwald were never applicable to any but the weakest solutions. Strangely enough, these leaders of the movement elected from the beginning to sail under the flag which has been so aggressively waved by Sir Victor Horsley of late and made their grog by dipping only the wetted stopper of the spirit bottle into a tumblerful of water. And over and over again they and their followers made the mistake of considering only the volume of the solution as a whole; consequently they always went wrong when dealing with concentrated solutions. No two moderately concentrated solutions prepared by their method were of comparable strength.

Daylight was first let into the subject when Morse and Frazer showed that simple and consistent results were arrived at, even in the case of concentrated solutions, by dealing always with a definite mass of water and varying the proportion of solute—by using weight-normal solutions, as they are now termed. Independently I introduced the same practice into my laboratory and gave my reasons for so doing in June 1906 in a communication to the Royal Society on the “Origin of Osmotic Effects.”

But how is the *thirst* of salted water—the superior activity

of composite electrolytes generally—to be explained, if not by the assumption that the solute is dissociated into ions? The expression “thirst” is used advisedly, as being far more appropriate than the term “osmotic pressure,” as the thirst experienced, for example, after eating salt junk is that engendered by the intrusion of salt into the water in the cells of the organism. The thirst originates in the salted water.

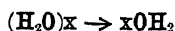
In 1885 and 1886 I insisted that both constituents of the composite electrolyte, water as well as salt, acted reciprocally; this view I have never departed from. The idea has constantly been present in my mind that the act of dissolution involves some form of conjunction—yet not hydration as commonly understood. I made this quite clear in 1887 in the British Association report on electrolysis, where the following statement will be found:

Nor do I conceive that it helps us to assume that a compound of hydrogen chloride with water is formed: it does not appear to me to be probable that an aggregate of the form $(\text{HCl})_y(\text{OH}_2)_x$ would be more susceptible of electrolysis than the component simple molecules and that these would be more likely to suffer dissociation when associated than when free.

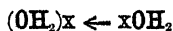
My work has so often been referred to in conjunction with that of Pickering on the determination of the composition of hydrates in solutions that it is desirable to point out that the questions considered by us were of a very different character. I always believed in the existence of hydrates but I was in search of something more—of a process, in fact, to account for the reciprocal character of the effect which solute and solvent exercised; one which at the same time would make it possible also to explain the effects produced by non-electrolytes. The explanation suddenly flashed into being after the discussion on ions in the Physical Section of the British Association in Bradford in 1900, the last occasion on which we enjoyed Fitzgerald's stimulating presence.

Taking into account the effect produced on water by a substance such as alcohol, for example (in lowering the freezing-point, etc.), as the extent to which such a substance could be supposed to exert a “dehydrating” action was very limited, it appeared obvious that its action must be “mechanical” rather than chemical; consequently, that the real change must be in

the water. A simple solution of the problem would be given, if it were supposed that water is a mixture of molecules of different orders of complexity, in equilibrium—a mixture of simple hydrone (OH_2) molecules with “polyhydrones” of various orders of complexity (see *Science Progress*, No. 11). The intrusion even of an entirely neutral substance (provided it were soluble) into such an assemblage would disturb the equilibrium in the direction represented by the generalised equation :



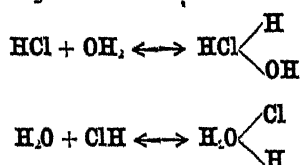
and the extent of the disturbance—the extent to which the polyhydrones would be dissociated—would depend on the number of individual neutral molecules introduced. To restore equilibrium in the direction



it would be necessary to cool such a solution more or less below the temperature at which ice would separate from pure water. The boiling point of the solution would be higher than that of water because of the presence of a larger proportion of *attractive* hydrone molecules and of molecules of the simpler polyhydrones. “Thirst” of hydrone would be conditioned in such a solution by the presence of an increased proportion of attractive hydrone molecules seeking to form water. The properties of aqueous solutions of non-electrolytes were thus referred to the fact that molecules of “Fair Hydrone” are eminently attractive of their own kind.

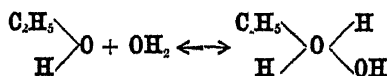
It thus became obvious that the method adopted by Van't Hoff and others of using volume normal solutions must be a fallacious one, except in the case of very dilute solutions, owing to the great variation in the relative molecular proportions of solvent and solute in such solutions, a substance of small molecular weight (such as alcohol) displacing but little water, one of high molecular weight (such as cane sugar) a large amount. It was clear that the solvent was the test substance—that all thought of analogy with the gaseous state must be put aside and the effect ascertained of this and that substance on this and that property of some definite mass of the liquid. Raoult's work, for example, had been done from such a rational point of view ; its real significance had been obscured, however, by the use of a false analogy.

It became necessary to revise the data relating to concentrated solutions from this point of view and especially to examine acids and salts under comparable conditions. The work carried out in my laboratory during the past few years has afforded material for what appears to be a rational explanation of the behaviour of substances the solutions of which behave as electrolytes. The assumption made is that the formation of composite electrolytes by the dissolution in water of substances such as the acids, the alkalis and salts generally, is a process involving the *distribution* of both constituents of the composite electrolyte—thus :



and that the occurrence of electrolysis involves the interaction of the two kinds of complex while under the influence of an electromotive force. The extent to which this distribution occurs determines the activity of the composite electrolyte.

In the case of non-electrolytes, it is to be assumed that only the water is distributed, thus :



The superior activity of salts, etc., in disturbing the properties of water is due to the fact that besides acting mechanically, they also exercise an attractive effect of their own ; in the case of a solution of hydrogen chloride, for example, the $\text{HCl} \begin{array}{l} \text{H} \\ \text{OH} \end{array}$ complex in virtue of the presence of the OH radicle, the $\text{H}_2\text{O} \begin{array}{l} \text{H} \\ \text{Cl} \end{array}$ complex in virtue of the presence of the Cl radicle, would both be active in much the same way that the hydrone molecule in water is active.

A completely distributed binary molecule AB would therefore exercise the effect of two molecules in disturbing the properties of the water. The further consequences of this hypothesis must be painted on another occasion.

My story has a moral. It is well known that the dissociation hypothesis dealt with in this essay has attained to an extraordinary popularity; this is in no way surprising, as chemical activity is a subject in which so many are interested.

All the major channels of communication and most of the minor are secured by the high priests of the cult: they command the almost universal obedience of student youth; and now their technical jargon confronts us everywhere. Thus, recently, when recovering from an indisposition, happening to take a mild course of Pharmacology, turning over the pages of a standard text-book on the subject, under "Acids" I came across the following explanation of the various physiological effects which they produce:

In all these effects mineral acids are much more active than organic acids . . . the difference in action is solely one of dissociation. All acids have an antiseptic action which varies with the dissociation, that is, the number of H ions in the solution.

This is but typical of the kind of statement constantly met with. It is either very learned talk in the mystic language of the initiate or misleading undiluted piffle—I should say the latter. The seriousness of the situation such language creates, however, lies in the fact that it is directed to those who later on will be responsible for the lives of men, who should grow up with clear ideas, to reverence science and to put trust in its votaries.

It will be held by some, perhaps even by many, that even if my indictment be true, it matters little nevertheless that a visionary scheme has been advanced—or even that it should have been forced into use for a time. It has inspired workers.

But at what expense of truth has victory been gained—if indeed there be true victory of any kind? What nature of example is it that we have set? To what extent are almost all sources of information available to the youthful mind polluted for years to come?

It is difficult to avoid the conclusion that we have offended against all the old canons of practice by which former workers were guided. They disputed, often vigorously and violently—they held the strongest opinions; but as a rule they were careful to balance arguments and to allow arguments to be balanced.

The modern method is not even to present the case of the opponent—the student is not allowed the choice of alternatives, he is rarely, if ever, informed that there are alternatives. Prof. J—— simply asserts: “This is truth; believe it you must and shall.” Of such kind has been the Leipzig message from the beginning. The spirit of intolerance is abroad among us. If we are not almost back to the days of the Inquisition, we are at least as dogmatic as are the adherents of any religious persuasion. And all this in the sacred name of science and of scientific method, of the discipline upon which we are placing so much hope of future enlightenment of society.

A large proportion of those who are being taught on such lines will become teachers in the future—the result must be disastrous; indeed, we can see already that such is the case. In the main the teaching of science is a failure, not only in our universities but also in our schools. The hopes formed by men like Charles Kingsley, Herbert Spencer, Huxley, Tyndall, are in no way being fulfilled. So little true scientific method is taught—the great lessons of truth to be learnt by the study of science, of which Kingsley speaks with such rare eloquence in his volume of *Scientific Essays*, is in no way brought properly before the notice of scholars.

The ideal philosopher contemplated by Faraday was one who pursues inquiry “zealously and cautiously, combining experiment with analogy, suspicious of preconceived notions, paying more respect to a fact than a theory, not too hasty to generalise and, above all things, willing at every step to cross-examine his own opinions both by reasoning and experiments.” This ideal is that which Faraday himself always had in mind and which gave to his work its incomparable value. We later workers may well strive to follow him.

The history of the development of the ionic dissociation hypothesis appears to me to be one of gravest warning, which we should heed before it be too late; the great ethical value of the lessons to be derived from it should not be overlooked.

SLEEPING SICKNESS

By G. D. PARKER, M.B. (LOND)

THE disease "sleeping sickness," more correctly African meningo-encephalitis, has for more than a hundred years been known to occur in West Africa. Until quite recently it ranked as one of the curiosities of medicine, but the widespread havoc that it has caused in the last ten years has made inquiry into the origin and course of the disease imperatively necessary. The history of this inquiry, illustrating as it does the modern method of investigation in such problems, will be of interest to more than purely medical readers who may not have the time at their disposal to read the admirable Report of the Sleeping Sickness Commission, to be found in the *Transactions* of the Royal Society from 1903 to 1907.

History of the Disease.—Winterbottom first observed sleeping sickness, and wrote a paper, "An Account of Native Africans in the Neighbourhood of Sierra Leone," in 1803. In 1868 Dumontier and Santelli wrote independent papers on the disease. Guérin wrote on it in the following year. The first complete and fairly accurate account was written by Corre, amongst the natives of Senegambia.

It has been stated that sleeping sickness has been observed among negro slaves imported to the West Indies, and also in Brazil; but the evidence is doubtful.

In 1891 there was a case of sleeping sickness in the London Hospital, and in 1900 two cases in the Charing Cross Hospital, sent from the Congo. The pathology of these cases, with their pathological history, was worked out by Mott, and the disease was found to be a meningo-encephalitis. More lately this malady has become commoner in various parts of Africa.

Geographical Distribution.—Sleeping sickness is endemic only in parts of equatorial Africa. Until very recent years it was limited to West Africa, from Senegal in the north to San Paolo de Loanda in the south. It has been known for many years on the Congo, has recently spread up the Congo to Stanley Falls,

and is commoner than has been generally supposed in the hinterlands corresponding to the above regions. On the Upper and Lower Congo the disease has lately occurred as an epidemic, and has been very widely fatal.

Cook, in 1900, discovered that sleeping sickness existed in Uganda; since then it has spread far, but has kept mainly to the north shore of Lake Victoria Nyanza. The disease apparently started in Usoga, spread west to Buddu and east to Kisumu. Northward, a few cases have been recorded at Kiadondo; but the disease is unknown in some of the northern provinces, in Unyoro, and round the Albert Lake and the Nile districts. The centre of its virulence has been in Usoga and Chagwe, round Entebbe, and in the islands close to the mainland, from the Sese group westward to the Kavirondo islands eastward. It is, apparently, not known in German East Africa to the south and west of the lake, nor among the Nandi and Masai tribes of British East Africa east of Kisumu.

Previously to 1901, sleeping sickness had been recorded only in West Africa. In that year a widespread epidemic occurred in Uganda. It was confined to the lake shores, and was most prevalent in low-lying areas, or in wooded districts near the water. The people living in populous centres were almost, and those on open grass plains quite, exempt from the disease. The Soudanese living in Uganda remain practically free from sleeping sickness. They live in large, well-laid-out villages, which are kept cleared of vegetation, and are regularly cleaned and swept.

Age has no influence on the disease. As regards occupation, the poorest people working in the fields suffer most. Chiefs and the better-class natives are less affected. Season and diet cannot be found to have any influence in the causation of the disease.

Etiology.—Many different hypotheses have been brought forward as to the origin of sleeping sickness. One theory was that the disease arose from the distress in connection with slavery. Other theories were—that it was a form of sunstroke, a variety of beri-beri, a variety of malaria, or due to scrofula. More recent theories are that the disease is a food-intoxication, that it is due to animal parasites, or due to bacteria.

Food-intoxication.—The theory has been put forward that this malady is due to manioc poisoning, and analogous to the disease pellagra, which is attributed to poisoning by maize.

Against this is the fact that the Uganda sleeping sickness has only appeared during the last three years [that is, previous to 1903], and that during that time no change of food has taken place among the natives.

Parasites.—Manson suggested the *Filaria perstans* as the cause of the disease. This has been disproved by the observations of Low, who finds that in British Guiana, where this parasite is common, there is no sleeping sickness, while in Kavirondo the opposite conditions are present. Forbes considered that the embryo of *Rhabdonema strongyloides* was the cause of the disease. This parasite penetrates the intestinal mucous membrane (Tessier), reaches the general circulation, and remains in the blood-vessels of the brain. Ferguson looked upon the *Ankylostoma duodenale* as the cause. As disproving this theory, it is found that the administration of appropriate anthelmintic remedies cures the patient of this parasite, but does not in any way permanently check the course of sleeping sickness.

Bacteria.—Cagigal and Lepierre in 1897 isolated a bacillus from the blood of a patient suffering from sleeping sickness; but their statement that this microbe, injected into animals, caused the disease was not afterwards confirmed. Marchoux believed the disease to be due to Fränkel's diplococcus, for the following reasons: He found Fränkel's diplococcus, post mortem, in the pericardial fluid of a case of sleeping sickness complicated by pericarditis. In another case, complicated by chronic rhinitis and suppuration of the frontal sinus, the nasal secretion was found to contain this microbe. Marchoux also noticed that sleeping sickness occasionally occurred in people who had had pneumonia.

Recently, the Portuguese Commission have described a diplostreptococcus constantly found, post mortem, in the cerebro-spinal fluid in cases of this disease. They have also often found the same microbe during life in patients suffering from sleeping sickness, in the cerebro-spinal fluid obtained by lumbar puncture. They state that this organism will not grow on gelatine, and not readily on the other common culture media. More recently still, however, the Portuguese Commission have stated that their organism will grow on gelatine and the other common culture media.

Dr. Broden, of the Léopoldville Bacteriological Laboratory,

describes a slightly motile bacillus, producing a pellicle on bouillon, growing freely on potatoes, found constantly in the blood in sleeping sickness, and never agglutinated by the blood in these cases. Castellani has found a streptococcus in non-complicated cases. His results were, however, in a number of cases negative.

Trypanosoma.—In November 1902 Castellani began to use a special technique, and since then he has often observed, in cases of sleeping sickness, a trypanosoma in the cerebro-spinal fluid and the blood, and in some cases special bodies which he looks upon as developmental forms of trypanosoma. In the cerebro-spinal fluid also, very minute bodies have been found, identical with the amœboid forms described by Plimmer and Bradford in *Trypanosoma Brucei*.

Predisposing Causes.—Sex and age do not exercise any influence on the incidence of the disease. Clarke speaks of circulatory disorders, mental depression, and want of food as predisposing causes. Any other causes of loss of resistance, such as parasites like *Filaria perstans*, *Ankylostoma deodenale*, etc., may act as predisposing causes.

Symptoms and Clinical Features.—The disease begins insidiously. First there is a slight mental change, next a disinclination for exertion, a tendency to sit about; then there are headaches and various transient pains, especially pains about the upper part of the chest. The facial expression becomes dull and apathetic. The disease may then run either an acute or a chronic course to its fatal termination. On examination, a patient with this disease is seen to present a dull, stupid appearance, with slowness of thought and ideas. The speech is slow, thick and indistinct, and the gait is shuffling. Headaches, dizziness and vague pains are complained of. The tongue sometimes presents a fine tremor, and in some cases there is also a fine tremor of the hands. The skin may be either soft and smooth or roughened and harsh. The glandular enlargements common among natives may become more noticeable. The temperature is raised. The evening temperature runs up from 101° to 102° F., with a fall to subnormal in the morning. There is often a daily range of 4° or more. The pulse is of low tension, and of a frequency of from 90 to 130 a minute. The temperature and pulse are very important as early diagnostic signs. The patient is very drowsy and lethargic. When taken

into hospital he improves for a few days, but soon begins to get worse again, becomes more drowsy, walks with increasing difficulty, and finally lies constantly in bed. Fine tremors become more marked—most noticeable in the tongue and arms. The skin may become rough and dull; eruptions are not common. The patient becomes weak and emaciated. The knee-reflexes, at first increased, become diminished, the motions are passed involuntarily, and saliva dribbles from the side of the mouth. The patient, becoming increasingly drowsy, is only roused with difficulty, and this condition gradually passes into coma. The temperature becomes constantly subnormal, convulsions occur in a few cases, and the patient passes into a condition of complete coma, and dies.

In an acute case, the disease runs its course in about a month or six weeks. In chronic cases, there is a slower development of symptoms, which may remain practically unaltered for considerable periods; but eventually they pass on into those of the later stages, and the disease ends fatally.

Pathological Anatomy.—The changes are those of chronic meningo-encephalitis and meningo-myelitis. The heart is flabby and pale. Patches of broncho-pneumonia are common. Congestion and œdema of the lungs are always present. The liver has the appearance of chronic malaria, and the spleen, much enlarged, often has a similar appearance, with characteristic pigmentation. The stomach is sometimes dilated, and it and the intestines occasionally show patches of congestion. Parasitic hæmatodes are always found in the intestines. In some cases *Bilharzia* ova have been found in the large intestine. *Tricocephalus dispar* is always found in the cœcum. Enlarged glands are found all over the body. Leucocytic infiltration is found on the meningeal surface of the brain. The association-fibres of the nerve-cells are diminished in number, and atrophied. The diagnosis in early cases is difficult, the most important fact being rise of temperature and increased rate of pulse. Later, the diagnosis is easy. The prognosis, as previously stated, is bad, this disease being always fatal.

Treatment.—Many drugs have been tried, but none with any permanent benefit. Mr. G. C. Low's investigations prove that *Filaria perstans* is not the cause of sleeping sickness. There are districts where *Filaria perstans* is common, yet sleeping sickness is absent. In other districts sleeping sickness occurs, and *Filaria*

perstans is absent. In some districts sleeping sickness and *Filaria perstans* are both present ; but the latter is found in the blood of the healthy as well as in that of the sick.

The report of Lieut.-Col. Bruce appears to show conclusively the following facts :

1. Sleeping sickness is caused by the presence in the blood of a trypanosoma.

2. This species is probably the *Trypanosoma gambiense*.

3. The cases of trypanosoma fever, described as occurring on the west coast of Africa, are probably early cases of sleeping sickness.

4. Monkeys are susceptible to sleeping sickness, and show the same symptoms, whether the trypanosomes injected are derived from cases of trypanosoma fever or from the cerebro-spinal fluid of cases of sleeping sickness.

5. Dogs and rats are partially susceptible, but guinea-pigs, donkeys, oxen, goats and sheep are absolutely refractory.

6. Trypanosomes are transmitted from the sick to the healthy by a species of tsetse fly, the *Glossina palpalis*, and by this only.¹

7. The distribution of sleeping sickness and that of *Glossina palpalis* correspond.

8. Sleeping sickness is a human tsetse fly disease.

The discovery of the trypanosome by Dr. Castellani was due to his introduction of the method of centrifuging the cerebro-spinal fluid. It has been found that the cerebro-spinal fluid in every case of sleeping sickness taken by lumbar puncture during life contains trypanosomes. The cerebro-spinal fluid taken by lumbar puncture during life from cases other than those of sleeping sickness does not contain trypanosomes. In the blood in cases of sleeping sickness trypanosomes are also invariably found to be present.

There is evidence to show that trypanosome fever is merely the first stage of sleeping sickness, corresponding to the period during which the trypanosomes are in the blood only, and have not gained entrance into the cerebro-spinal fluid. No difference can be found microscopically between the trypanosome of trypanosome fever and that of sleeping sickness. It is found that the tsetse flies of East Africa are capable of conveying the

¹ Further evidence, as will be mentioned later, seems to show that other species of *Glossina* may convey the disease.

trypanosome of sleeping sickness after eight to twenty-four hours.

Further evidence has been collected, which shows :

1. That sleeping sickness is at first a polyadenitis caused by the *Trypanosoma gambiense*.

2. That, in addition to enlargement of the glands, the blood shows a constant lymphocytosis at all stages of the disease.

3. That sleeping sickness itself is the last stage of the disease, and is always fatal. It consists in a polyadenitis, together with signs and symptoms of changes in the nervous system, and the onset of these symptoms coincides with the entrance of the trypanosoma into the lymph spaces of the nervous system, accompanied by an increase of the mononuclear elements of the cerebro-spinal fluid.

4. That the resistance both of men and monkeys to the *Trypanosoma gambiense* varies greatly in different cases, and that, apparently, a certain number of both acquire sufficient immunity to arrest the development of the disease at the early stage.

5. That the action of arsenic on the disease is only partial. It destroys a number of trypanosomes, and these probably act as immunising agents. In the polyadenitis, arsenic increases the natural resistance.

6. That bacterial invasion, mainly by cocci, occurs in some cases, but only at the very last.

7. That, in addition to the *Trypanosoma gambiense*, other trypanosomes occur in Uganda, probably pathogenic to animals.

8. That these other trypanosomes differ widely from *Trypanosoma gambiense*.

9. That one of these trypanosomes is probably identical with *Trypanosoma Brucei*.

10. That these different trypanosomes are conveyed from the sick to the healthy by the Uganda tsetse fly (*Glossina palpalis*) and other species of *Glossina*.

In Uganda at the present time, in the sleeping sickness areas, from 50 to 75 per cent. of the inhabitants have polyadenitis, but are capable of doing their ordinary work, and have few symptoms; these individuals act as reservoirs for the trypanosome.

According to their after-history, these cases may be divided into two classes :

1. The patients develop sleeping sickness, or die of pneumonia or some other intercurrent affection.
2. They remain in good health for a long period, and seem to develop a tolerance for the trypanosome.

The question is whether any of these acquire sufficient immunity to destroy the parasite, and whether this immunity can in any way be artificially increased.

A curious condition has been found in the stomach in cases of sleeping sickness, there being areas with a dark-red centre and light-red periphery, consisting of masses of petechial hæmorrhages. The lymphatic glands in all cases of sleeping sickness are enlarged, and the juice taken by puncture during life contains many active, and also numbers of disintegrating trypanosomes. In the cerebro-spinal fluid taken during life are found lymphocyte cells, occurring in greater numbers the more advanced the case. In a certain proportion of cases in the last stage, the gland juice becomes infected with bacteria, especially diplostreptococci.

From the investigations made, it is now clear that all the essential features found in trypanosome or trypanosoma fever are found in sleeping sickness, that many of the cases end fatally as sleeping sickness, that a certain number die of intercurrent affections, especially pneumonia, and that a certain proportion show a considerable degree of tolerance to the parasite. Apparently some of the last class of cases develop sufficient resistance to destroy the trypanosoma.

As regards the habits of *Glossina palpalis*, which carries the infection of sleeping sickness, it has been found :

1. That this fly abounds where there are trees and bushes near water, and that sleeping sickness occurs in these places.
2. That sleeping sickness has spread from Uganda and Usoga east and south.
3. That there is no sleeping sickness east of a line drawn from the Maragoli hills down the Maragoli stream to the bay, and across the bay (Kavirondo) to Homa. This line is found also to be the eastern limit of the tsetse fly, with the exception of the Port Florence district.
4. That the Kuja is the only river that carries the tsetse fly inland, and is also the only river that has trees at its mouth and thick vegetation along its course.

From experiments on animals, it appears that the *Glossina*

palpalis can convey trypanosome disease set up by several varieties of trypanosomes from sick to healthy animals. It appears from this also, that other varieties of *Glossina* besides *palpalis* can convey the *Trypanosoma gambiense*. These flies tend to "abort" when kept in captivity.

Experiments prove that the *Trypanosoma gambiense* multiplies in the stomach of *Glossina palpalis*. It is found that infection cannot be produced by inoculation of trypanosomes from the intestine of the *Glossina*.

The etiology of sleeping sickness has now been conclusively established by the accumulation of the following facts:

1. The death of Europeans suffering from *Trypanosoma gambiense* infection long after they have left the country where it is endemic.
2. The production of the characteristic lesions in monkeys by experimental inoculation.
3. The absence of the lesions in any other conditions of infection.
4. The chronicity of the disease as shown by European cases.
5. The existence of cases of sleeping sickness only when *Trypanosoma gambiense* and *Glossina palpalis* coexist.

It has been found that there is a parallelism between the intensity of the lethargy, the chronicity of the disease, and the characteristic histological changes in the central nervous system. The disease is characterised by a chronic polyadenitis (Greig), followed by a chronic inflammatory change in the lymphatics of the brain and spinal cord. In practically all cases the cervical lymphatic glands are enlarged, and the most chronic change is found about the base of the brain. It is therefore probable that the chronic inflammation of the lymphatics spreads along the nerves, spinal ganglia, and nerve-roots to the central nervous system, and especially along the lymphatics of the nerves and vessels entering the base of the skull. The chronic inflammatory change in the nervous system consists in a proliferation and overgrowth of neuroglia cells, especially those related to the subarachnoid space and perivascular lymph spaces of the brain and spinal cord, with accumulation, and probably also proliferation, of lymphocytes in the meshwork.

Preventive and Precautionary Measures.—Sleeping sickness may be carried wherever an infected person travels. The disease may spread wherever *Glossina palpalis* exists. Most of

the ordinary preventive measures are inapplicable in dealing with this terrible and fatal disease. Quarantine is impossible. Aggregation of the sick for isolation in large numbers is difficult and of little use. The most practical method is removal of the sick in each community from direct communication with the fly-range. Where carried out thoroughly, this would stamp out the disease, and it should be primarily enforced in the camps and villages of the main lines of communication.

The importance of these preventive measures will be seen when the failure of the medical treatment is considered. When once the disease has gained a footing, it has, more or less delayed, only a fatal termination.¹ It is to be hoped that the experiments now in progress will give a more effective remedy, but as far as our present knowledge extends, prevention is the only way of dealing with the disease.

LIST OF REFERENCES

Most of the references quoted above are to be found in the Report of the Sleeping Sickness Commission, published in the Royal Society's *Transactions* from August 1903 to February 1907. The Reports are in eight parts, numbers i. to viii. References below in Roman figures refer to the number of the Part, and in Arabic figures to the number of the article referred to.

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Report by Cuthbert Christy, M.B., C.M. III. 6.

Report by Lieut.-Col. David Bruce, R.A.M.C., F.R.S., David Nabarro, M.B., and Capt. E. D. W. Greig, I.M.S. IV. 8.

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Report by Capt. E. D. W. Greig, I.M.S., and Lieut. A. C. H. Gray, R.A.M.C. VI. 11.

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¹ Since the above was written three cases have been reported as cured after the administration of Liquor Arsenicalis (Fowler's solution). "Rapport s. l. Travaux à Lab. d. l. Soc. Belge, etc., à Leopoldville," Bruxelles, 1908. See also *Brit. Med. Journ.*, March 13, 1908, p. 681.

PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES

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SOLUTRIAN MAN AND THE BUSHMEN

THE upper palæolithic epoch is characterised throughout by the fauna of the mammoth: at first, in the Solutrian age, the horse, mammoth, and bison are the predominant animals, but later, in the Magdalenian, the reindeer plays a more important part.

The fauna of the mammoth is often spoken of as the cold fauna, in contradistinction to the fauna of *Elephas antiquus*, or the warm fauna. It is generally difficult to draw very precise inferences from fauna to climate: the mammoth was certainly well fitted to withstand cold, but it roamed over a very wide area; its range was possibly determined less by temperature than the distribution of the plants upon which it fed. The remains of plants found in the stomach of the mammoth discovered in 1901 included a species of *Carex*, *Thymus serpyllum*, *Papaver alpinum*, and *Ranunculus acris* var. *borealis*, all seed-bearing species; in a previous find pine needles are said to have been found between the teeth. The horse has been supposed to indicate extensive prairies, but Prezevalsky's wild horse, which existed in upper palæolithic times, now inhabits the great Dsungarian desert, between the Altai and Thian-Shan Mountains. It is said to prefer the saline districts, and to be able to go a long time without water. The bison was widely distributed over Europe in early historical times, and it now survives in Lithuania; the American bison roamed the prairies. The reindeer is at present confined to the Arctic regions of both hemispheres; it flourishes best in a cold, dry climate. In winter it finds shelter in the woods, and does not venture into the low, treeless plains except in summer. Another cold-loving animal of the period is the musk ox, which now inhabits the Arctic parts of North America and Greenland. Lieutenant Greenly found it in Grinnell Land as far north as lat. $81\frac{1}{2}^{\circ}$ N.

The geography of Europe underwent considerable change in the course of the palæolithic age. The conditions described as existing at its commencement did not persist into the upper palæolithic, the continental area was at first gradually reduced to narrower limits, but later on the sea again withdrew, and an approach was made to the earlier state of things. About the beginning of the upper palæolithic, however, a fresh advance of the sea occurred, to be followed later by a final withdrawal, and the establishment of the existing coast-lines.¹

Since G. de Mortillet first subdivided the upper palæolithic into the Solutrian and Magdalenian, repeated efforts have been made to overturn these two systems. The Eburnian or Papalian of Piette corresponds approximately with the Solutrian, and his Cervidian or Gourdanian with the Magdalenian. Some authors separate off the basal part of the Solutrian as a distinct series—the pre-Solutrian or Aurignacian.²

The distribution of Solutrian stations is indicated on the map (fig. 1). The remains of Solutrian industry are found not only in caves, rock-shelters, and river gravels, but also in the löss. One of the most instructive caves, famous as affording a key to classification, is La Grotte du Placard (Charente). In this no less than eight layers containing the relics of human occupation occur, separated by intercalated sterile beds varying from 7 in. to 2 ft. in thickness. The lowest layer is Mousterian; above it lies (2) the lower Solutrian, and (3) the upper Solutrian; next (4 and 5) the lower Magdalenian, and (6 and 7) the upper Magdalenian, and finally (8) the Robinhausian.

The löss is a yellowish grey or brown deposit of unstratified sandy and calcareous loam, often much broken up by joints and traversed by narrow almost vertical tubes. Curious calcareous concretions, known locally as "löss manchen," are scattered through it in discontinuous layers. It rarely contains fossils, except of land snails, such as *Helix hispida*, *Pupa muscorum*, and *Succinea oblonga*. Maintaining a thickness of from 10 to 60 metres, it spreads over a great part of Europe as a very

¹ See Marcellin Boule, "La grotte du Prince," *L'Anthropologie*, 1906, xvii. p. 257.

² H. Breuil, "Essai sur la stratigraphie du dépôts de l'âge du Renne," *L'Anthr.* 1905, xvi. p. 511. A. Rutot, "Le Présolutrécien ou Aurignacien en Belgique," Congrès Préhistorique de France, 1907, p. 179. A. de Mortillet, "La grotte du Placard et le niveau d'Aurignac," *C. R. l'assoc. française*, 1907, places the horizon of Aurignac above the Solutrian; H. Breuil, "La question Aurignacienne," *Rev. préhistorique*, 1907, places it below.

irregular fringe to the boundaries of the ancient and vanished ice-sheets. It owes its formation to the wind, which, during a genial episode, swept the fine dust brought down by the glacial rivers over the grass-grown steppes of the period. As the dust accumulated the grass struggled upwards to maintain its existence, and the vertical pipes in the loess were left by the decay of its roots. There is an older and a younger loess; the older overlies the third shotter terraces of the last genial period, antecedent to the last glacial age. It is said to be in this

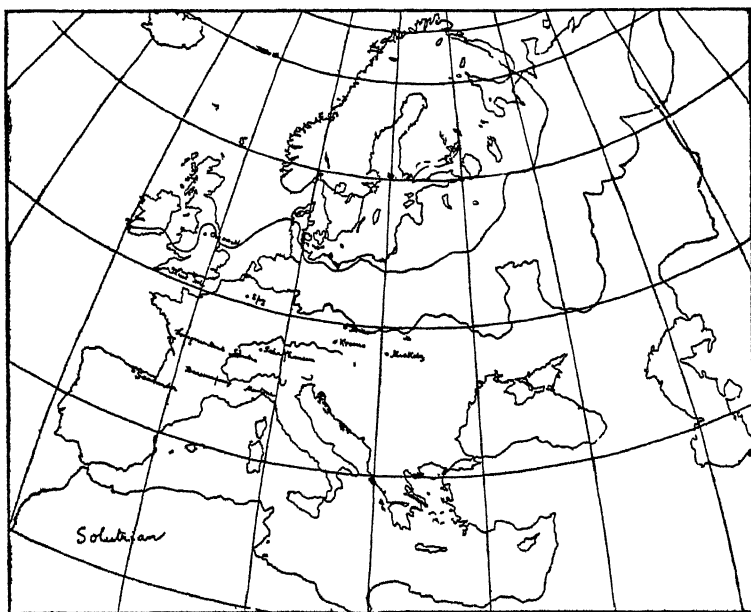


FIG. 1.—Map showing Solutrian Stations in Europe.

older loess that Solutrian remains are found. The most famous localities are near Krems, on the Danube; Brunn and Predmost, in Moravia. Stations also occur in Bohemia and Hungary.

Solutrian Man.—As we enter upon the Solutrian stage we become aware of an extraordinary advance in the arts. The fabrication of flint instruments attained at this time a perfection which the archæologist can only regard with astonishment. Some of the best work seems to anticipate that of the neolithic epoch. After culminating towards the close of the period, deterioration set in and persisted till a fresh climax was reached in comparatively recent times.

The Solutrian flakes are not only remarkably thin, but minutely and elaborately dressed on both sides. This combination of characters suggests the introduction of some new method of flaking. Previously produced by blows, it would now seem to have been effected by pressure. It is difficult otherwise to understand how such delicate laminae could have received their finished form. Travellers have from time to time brought home accounts of the employment of this method by existing races; thus Krause¹ in describing the Fuegians, who are very expert in the dressing of flint, remarks that the fine flaking or retouches are made by pressure skilfully applied by means of a piece of bone—an old harpoon deprived of its point often serving this purpose. The Eskimos of Alaska, who employ the same process, make use of a piece of reindeer's horn mounted in a handle of fossil ivory.

The outlines of the Solutrian flakes, though simple, are often extremely elegant. Some forms are especially characteristic, such as the laurel-leaf point (*pointe en feuille de laurier*), the willow-leaf, and the shouldered point (*point à cran*). These are not always found associated in the same Solutrian deposit, and are sometimes absent altogether, even when other indications point to a Solutrian age.

It need scarcely be added that the Solutrian flint implements are far from maintaining a uniform level of excellence. Not only did the workmen differ in skill, but the primitive hunter was accustomed to adapt means to ends. He was not the man to "cut rocks with a razor," and when a flake was required at once for some simple operation, it would be made on the spot, *ad hoc*, and thrown away when it had served its purpose. There seems to have been a kind of trade in the best implements; this at least is suggested by the discovery of caches of laurel-leaf and shouldered points, as at a locality near Volgu (Saône et Loire), which afforded a collection of fine examples made from a kind of flint not found in the neighbourhood.²

A second advance in the palæolithic industry is marked by the introduction of a new material. A use had been found for bone, which, while tougher and less brittle than flint, is capable of taking a fine point, and to obtain this recourse might be made to the operation of grinding. From the grinding of bone to

¹ E. Krause, *Zeits. f. Ethn.* 1903, xxxv. p. 537.

² Chabas, *Les Silex de Volgu, Chalon*, 1874.

that of stone does not seem a great step, but it was not taken till long afterwards, in the neolithic period.

At first bone implements are very rare and simple in form, but accompanying them are objects sculptured in the round or in low relief, of which we shall speak later.

Allusion has already been made to the life of the period. Europe at this time evidently teemed with game, which afforded a rich prey to the Solutrian hunters. At Solutré itself, a station in the Rhône valley a little to the north of Lyons, where the horse seems to have been a favourite food, the broken bones of these animals, left as the refuse of many feasts, form a mass of breccia considerably over 100 yards in length and 10 ft. high; and at Predmost, in Moravia, where the mammoth was the chief victim, more than two thousand of its molar teeth have been found gathered together in a mass of débris. In connection with this we may recall the observations made by Captain Harris when travelling in South Africa :

“In many places,” he writes, “the ground was strewn with the blanched skeletons of gnus and other wild animals which had evidently been slaughtered by Bushmen, and traces of these troglodytes waxed hourly more apparent as the country became more inhabitable. The base of one hill in particular, in which some of their caves were discovered, presented the appearance of a veritable Golgotha; several hundred skulls of gnus and bonteboks being collected in a single heap.”¹

Under such favourable conditions life seems to have afforded Solutrian man a certain amount of leisure. At all events, his energies were not wholly devoted to the chase, and we now witness the birth of the fine arts. Sculpture, painting, drawing successively make their appearance, and the best examples attain so high a pitch of excellence that enthusiastic discoverers have spoken of them as superior in some respects to the work of the Greeks. Sculptures in the round and in low relief were the first to attract the attention of observers; but in the course of the last twenty years a series of remarkable discoveries have brought to light whole picture galleries of Solutrian age. The first to set eyes on these was a Spanish nobleman, Marcellano de Sautualo, who when visiting the International Exhibition in Paris of 1878 became acquainted with the discoveries made in the caves of Southern France, and was thus led to investigate

¹ G. W. Stow, *The Native Races of South Africa*, London, 1905, p. 85.

some caves which exist near his own home at Santander. In one of these, the cave of Altamira, he found the usual palæolithic débris, bones of extinct animals, and worked flints, among them a laurel-leaf Solutrian point of coarse workmanship. But in addition he perceived on the walls of the cave and even on the roof a crowd of figures, some of life size, representing horses, deer, bison, and other animals, faithfully depicted in a great variety of attitudes. M. de Sautuola lost no time in bringing this surprising discovery before the Archæological Congress of 1879, and published a full description in 1880.¹ It was received with the most profound scepticism. Subsequently M. L. Chiron observed outline drawings on the walls of a cave in the Ardèche, known as the Chabot, and his discovery was subsequently confirmed by Prof. Capitan. A few years later (1895) similar drawings were found by M. Rivière in the cave of La Mouthe,² and in the following year by M. François Daleau in the cave of Pair-non-Pair in the Gironde.³ In the Solutrian layer of Pair-non-Pair, M. Daleau found the red oxide of iron which had furnished the pigment for the paintings on the walls, as well as the pestles of granite and quartzite which had been used for pounding it up, and several scapulæ daubed with red which seemed to have served for palettes.

These fresh discoveries did not produce conviction. When M. Rivière submitted his results to the Archæological Congress in 1897, they met with much unfriendly criticism. Yet the author had made a strong case; for he pointed out that some of the figures are covered by a fairly thick layer of stalactite; that the red clay which forms the floor of the cave extends above the lower part of some of the drawings so as to conceal the feet of some of the animals depicted; and finally, that in their style, boldness of characterisation, and even in their faults they closely resemble the palæolithic drawings which had long been recognised on bone or ivory.

In a sympathetic review, written in the following year, M. Marcellin Boule⁴ asserted that the arguments which had

¹ M. de Sautuola, *Breves apuntes sob algunos objetos prehistoricos de la provincia de Santander*, Santander, 1880, 8vo, 28 pp. 4 plates.

² E. Rivière, "La Grotte de La Mouthe," *Bull. Soc. d'Anthr.* Paris, 1897, pp. 302, 484, 497.

³ F. Daleau, "Les gravures sur rocher de la caverne de Pair-non-Pair," *Actes de la Soc. Archæ.* Bordeaux, 1897.

⁴ M. Boule, "La grotte de La Mouthe," *L'Anthr.* 1898, ix. p. 676.

been opposed to the views of M. Rivière were without validity. At the same time, he hesitated to commit himself to a definite opinion.

It was not till 1901 that the general incredulity began to yield, partly in consequence of fresh discoveries by Prof. Capitan and the Abbé Breuil, who described drawings and paintings from additional caves.¹ At the same time M. Rivière furnished fresh evidence from the cave of La Mouthe,² and M. Marcellin Boule, in a review³ of the work of these authors, now recognised its convincing force. Finally, M. Cartailhac, who had been previously one of the most uncompromising opponents of the genuineness of the alleged discoveries, courageously admitted that he had been mistaken.⁴ All doubts were now dispelled, and the subsequent progress of investigation has been accompanied by continually increasing discovery.⁵

In giving a brief account of these drawings we cannot do better than commence with the cave of Altamira, the starting-point of all subsequent discoveries. A plan of the cave is given below (fig. 2), and a reference to it will save a lengthy description. The finest collection of figures occurs on the roof

¹ Capitan and Breuil, "Une nouvelle grotte avec parois gravées à l'époque paléolithique," *C. R.* September 16, 1901; and "Une nouvelle grotte avec figures peintes sur les parois à l'époque paléolithique," *C. R.* September 23, 1901.

² E. Rivière, "Les dessins gravés et peints de la grotte de La Mouthe," *Rev. Sci.* October 19, 1901.

³ M. Boule, "Les gravures et peintures sur les parois du cavernes," *L'Anthr.* p. 671.

⁴ Emile Cartailhac, "Les cavernes ornées de dessins: Le grotte d'Altamira, Espagne; 'Mea Culpa' d'un sceptique," *L'Anthr.* 1902, xiii. p. 348.

⁵ E. Cartailhac and H. Breuil, "Les peintures et gravures murales des cavernes Pyrénéennes," I. Altamira (à Santillane, Spain), *L'Anthr.* 1904, xv. p. 623; II. Marsoulas, près Salies-du-Salat, Haut-Garonne, *L'Anthr.* 1905, xvi. p. 431; III. Niaux (Ariège) *L'Anthr.* 1908, xix. p. 15; Capitan, Breuil, and Peyrony, "Les figures gravées à l'époque paléolithique sur les parois de la grotte de Bernifol (Dordogne)," *Rev. de l'École d'Anthr.* Paris, 1903, p. 367; H. Breuil, "L'évolution de l'art pictural et de la gravure sur murailles dans les cavernes ornées de l'âge du Renne," *L'Anthr.* 1905, xvi. p. 513; Peyrony, "Nouvelles recherches sur la grotte des Eyzies," *L'Anthr.* 1905, xvi. p. 515; Capitan, Breuil, et Ampoullange, "Une nouvelle grotte préhistorique à parois gravées," abstract, *Rev. de l'École d'Anthr.* Paris, 1904, x. p. 320; Capitan, Breuil, et Peyrony, "Une nouvelle grotte à parois gravées, La Calvitie (Dordogne)," *Rev. de l'École d'Anthr.* Paris, 1904, p. 379; Hermilio Alcalde del Rio, *Las Pinturas y Grabados de las Cavernas prehistoricas de la Provincia de Santander*, Santander, 1906; H. Breuil, "Cavernes espagnoles peintes et gravées," *L'Anthr.* 1906, xvii. p. 625.

of the recess (c) near the entrance. The earliest efforts are drawings in black outline, some of which could scarcely be better; these were succeeded by paintings in red wash, which are somewhat crude; then follow incised drawings, traced with a sure hand, and showing no signs of retouching. Last of all come the polychromes, which are rudimentary to begin with, but subsequently attain a high degree of perfection.

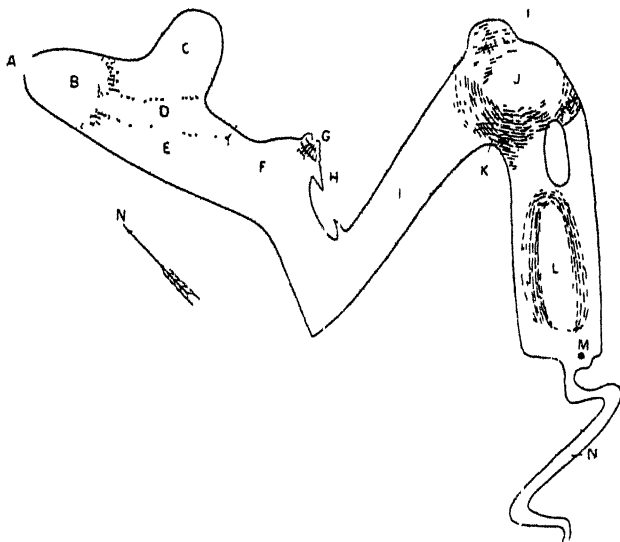


FIG. 2.—Plan of the Cavern of Altamira. Drawn by M. Harlé.

Total length, 280 meters. A, entrance; B, vestibule half filled with kitchen debris and fallen fragments from the roof; C, chamber on the left, 40 meters long by 10 meters broad, with large paintings; D, fallen rocks; E, gallery on the right opening into the chamber F, from which a cascade of stalagmite, G, covered with sculptures, descends to the left; H, a narrow diverticulum, with red figures on the walls, opening into I; I, gallery, with floor covered by fallen fragments from the roof; J, chamber with vaulted dome-like roof; K, cascade of stalagmite; L, elongate, nave-like chamber; M, shallow water pits; N, terminal passage. The figures occur over all the walls, but mostly on the roof of the chamber left of D.

The greater number of the animals shown in fig. 3 are polychromes of this kind. Where these occur there is evidence to show that the surface was prepared for their reception, previously existing paintings having been washed or scraped off. The outlines were first drawn in with black pigment, then the colours were put on, tufts of hair on the mane and elsewhere being indicated by touches with a brush; the body colour was smeared on as a soft paste, extended and graduated to give the half-tones, and then retouched by washing and scraping, bands of colour being removed to give the high lights and to



FIG 3—Outline Drawings of the Polychromes on the Roof of Chamber C
By l'Abbe Breuil

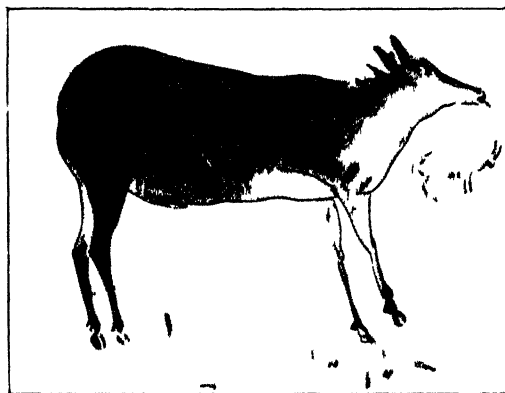


FIG 4—The Deer seen on the extreme left of Fig 3. From a pastel copy by
l'Abbe Breuil. The original polychrome is 2.20 meters in length.



FIG 5—Sketches of the Human Face from the Cave of Marsoulas. Copied
by l'Abbe Breuil

bring the limbs out against the body. Advantage was frequently taken of the irregularities of the walls to give an effect of relief to the whole figure. The colours employed were red, brown, black, and several shades of yellow, graduated into numberless half-tones and tints. The pigments from which they were obtained are iron ochre and oxide of manganese.

M. Cartailhac and the Abbé Breuil speak in enthusiastic terms of the group of polychrome figures shown in outline in fig. 3; they characterise it as "*l'œuvre la plus parfaite que nous puissions actuellement citer de ces époques reculées, et qui place les vieux peintres des âges glyptiques bien au-dessus des animaliers de toutes les civilisations de l'orient classique et de la Grèce: rien n'égale la rigueur du trace, l'exactitude et la hardiesse des attitudes, l'habileté et le fondu des nuances rouges, brunes, noires, et jaunes qui se mélangent et se graduent en mille demi-teintes.*"

It will be noticed that the animals are irregularly scattered; they are full of character and life, but they tell no story. The greater number are bisons; standing, walking, rampant, they crowd the middle of the picture: on the extreme left is a deer, shown on a larger scale in fig. 4; above it to the right is a wild boar, probably one of the animals most dreaded by Solutrian man; next to this is a horse with its colt; on the extreme right is another wild boar, apparently in the act of charging. In other caves additional animals are represented: the mammoth, sometimes full grown, sometimes very young, looking like a ball of fur; and the woolly rhinoceros, with its two great horns one behind the other.

Besides animal forms, numerous other not very intelligible drawings occur on the walls, some spoken of as "*scutiform*" and "*tectiform*," which in the character of their detail recall the markings on some of the Australian "*churinga*," and others which appear to be conventional symbols, but as they are not arranged in order and seldom repeat the same forms it is difficult to extract any meaning from them. Impressions of the human hand are met with, painted in red in Altamira, but in other caves also in black, and sometimes uncoloured on a coloured ground. These seem to be older than any of the other markings.

We cannot survey the series of pictures with which Solutrian man has illustrated the animal life of his time without a feeling

of delight, and the pleasure we feel in this glimpse of a vanished fauna is enhanced by the fact that we look at it through the eyes of Solutrian man himself. The pictures seem to be a pure study of nature, expressing the vivid sympathy of the artist with the world around him. In part this must be so, but there may be more. Without a full understanding of the civilisation of a race we cannot understand its art. Our own minds are saturated with the influence of our age, and the art of Solutrian man may have meant something very different and something much more to him than it does to us. Indeed, M. Salomon Reinach has endeavoured to show it was intimately bound up with his religion.¹ He points out that all the animals represented are such as are desirable for food: "undesirable" animals, such as lions, bears, and tigers, are never depicted. But it is a widely spread belief, once apparently universal, that the image of an object gives the possessor some sort of hold upon it, and thus by drawing the likeness of these animals primitive man might have thought to influence them in the chase. When we speak, M. Reinach remarks, of the magic of the artist's pencil, we use a metaphor which had once a literal meaning. Again, in the initiation ceremonies practised among the Australian aborigines a sacred figure, which the women and uninitiated are not permitted to see, plays an important part; and in connection with this the singular fact is cited that the animal figures in the caves never occur in the better illuminated parts, but always at some distance from the entrance, where the obscurity is so great that nothing can be seen by civilised eyes without the aid of artificial light. At the same time no signs of smoke remain to show that the troglodytes made use of torches or similar means of illumination.

The portrait that we should most welcome is not to be found on the walls of the caves, for Solutrian man has not depicted himself. There are some grotesques (fig. 5) which seem to be meaningless, like the foolish caricatures on a schoolboy's slate; possibly they are intended for demons, which the Babylonians are said to have made as unprepossessing as possible in order that they might be frightened at their own image. Some singular beings are also represented, which have been taken for anthropomorphous apes.²

¹ S. Reinach, "L'art et la magie apropos des peintures et des gravures de l'âge du Renne," *L'Anthr.* 1903, xiv. p. 257.

² Piette.



FIG. 6—Bushman Painting, representing Hanks chased by Lions. After Stow

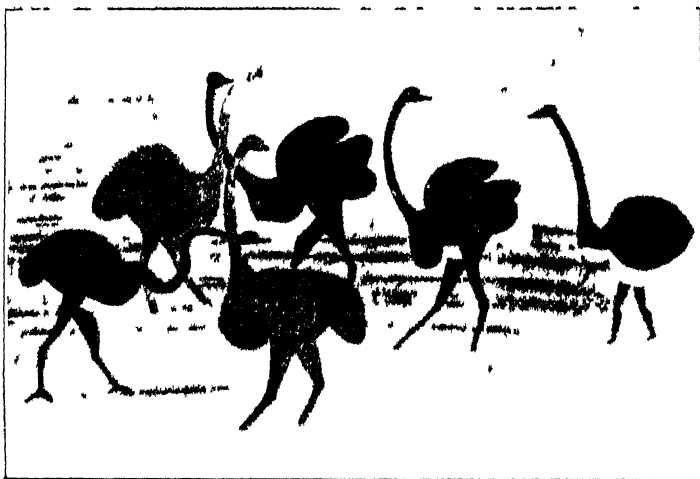


FIG. 7—Bushman hunting Ostriches disguised as one of them

In their apparent neglect of the human form the Solutrian men resemble the Ainos of Japan, who decorate the rods used to lift the moustache when drinking with figures of birds, mammals, and fish, but never of men; and when asked the reason for this omission they assert that they do not know how to represent the human form. It is difficult to accept this statement as sufficient.

We must now set out on a rather difficult quest. What, we may ask, has become of this gifted Solutrian race? Has it wholly vanished out of ken, either by extinction, or by transformation into a more civilised people, or by absorption into some conquering race, like say the Egyptians, or does it possibly still survive, retaining more or less its primitive characters?

In attempting to answer this question we may begin by confining our attention to the mural paintings and endeavour to discover whether there is any existing race which practises the same art. Drawing seems indeed to be almost as universal as speech: the Tasmanians could trace rude outlines of objects that excited their interest, the Australians and the Indians of California make impressions of the human hand on the walls of caves, and the Australians even sketch outlines of men and animals, but these are extremely crude, and never rise to the same level as the artistic sketches of palæolithic man. There are no monochrome paintings in the caves of Australia, still less anything resembling the wonderful polychromes. The art of the ancient Mexicans was so different that it can hardly be brought into this comparison; that of the Egyptians makes a nearer approach, but it stands on a still higher plane. Africa, however, furnishes us with another people, still in much the same stage of culture as the Solutrian, inhabiting caves, and decorating the walls with paintings, both monochrome and polychrome, some of which recall in the closest manner the best efforts of Solutrian times.¹ These are the Bushmen, a race which once spread over a great part of South Africa, but now maintains an unequal struggle for existence in the Kalahari desert. Most of their paintings represent scenes from the chase; in the accompanying illustration (fig. 6) a group of elands is shown attacked by lions. A good deal of the original effect is lost by the translation of the various tints into black and white, but it will be perceived that the outlines are firmly

¹ Impressions of the human hand are also met with on the walls of these caves.

and correctly drawn—those indeed who are familiar with the eland speak of this as a perfect representation.

In the next figure (fig. 7) the five birds to the left represent real ostriches; that which seems to be one on the right is a Bushman disguised as an ostrich; the extended bow betrays him. The colours in this are not so nicely graduated as in the preceding, but it is a good picture, the outlines are well drawn, the attitudes of the birds true to life, and the grouping is extremely skilful. A Solutrian could scarcely have done better.

There are obvious differences between the Solutrian and the Bushmen paintings: in the latter the various figures are not thrown on to the wall in a disorderly crowd, but are grouped together into a picture, which tells a tale of its own; neither is the human figure excluded—on the contrary it often plays a pre-dominant rôle. At the same time the differences are outweighed by the similarity, the technique is much the same, there is the same realistic truth, and the same quality of movement in the animal forms. Certainly of all existing hunting tribes the Bushmen make the closest approach in their art to that of the Solutrian age. This at least may be affirmed. If we assume as a working hypothesis a close alliance between these two schools of painting, can we advance a step farther and assume that the Bushmen and the Solutrian race are closely connected by blood? By no means. We cannot argue from identity of cult to identity of race; the error of the philologist in treating a communicable character as an inborn gift has caused trouble enough in this respect, and we cannot be sufficiently on our guard against it. But there is no reason why we should not continue our inquiry, and as a next step seek for evidence of another kind, this time anatomical. If we attentively examine the Bushmen as they are represented in their paintings, we shall perceive a peculiarity in their outline, owing to that excessive development of one feature which is known as *steatopygy*. Direct observation of existing Bushmen shows them to be *steatopygous* (fig. 8); the Hottentots are still more so. In the women of these races this character is associated with another, that is, a remarkable elongation of the *labiæ minoraë*, so that they are sometimes spoken of as *longinymph*. European women are sometimes *longinymph*, but not *steatopygous*; the association of these two characters is peculiar to the Bushmen, Hottentots, and perhaps the Accas. The greater the

development of these features, the greater the approach to a Hottentot's ideal standard of beauty.

If now we return to Solutrian man we shall find that although for some inscrutable reason he refrained from depicting the human form, he had no scruple against sculpturing it in the round; indeed, he took a special pleasure in carving figurines, which almost invariably represent woman in the nude. A considerable number of these have been discovered in various caves, as at Laugerie-Haute and Brassempouy, Barma grande (Mentoni), Pont-à-Lesse (Belgium), and in the löss at Predmost (Moravia); at least a dozen are preserved in the Museum at St. Germain near Paris. They are of unequal merit; some are extremely crude, others, however, are true works of art, and well deserve the praise bestowed upon them by M. Salomon Reinach, who remarks that there are at least two examples among them which by their realism and intelligent rendering of the female form are superior to all the artistic productions of the Ægean and Babylonia.¹ They have been closely studied by M. E. Piette, who divides them into two groups, one modelled from a race which it is difficult to identify, and the other presenting just those characters which we have enumerated as peculiar to the Bushmen, Hottentots, and Accas. Thus as early as 1895, before the mural paintings of the caves had been recognised as genuine, M. Piette was able to assert that if we seek for the nearest representatives of the people represented by these models, we shall find them among the Bushmen.²

Certainly the artists who carved the figurines have shown in the clearest manner that they were intimately acquainted with women who presented a close anatomical resemblance to the existing Bushwomen, and the presumption is that these were women of their own race.

The supposed connection between Solutrian man and the Bushmen begins to acquire an appearance of probability and the case is still further strengthened by the discovery of the actual remains of Solutrian man himself. The Grotte des

¹ S. Reinach, "Statuette de femme nue découvert dans une des grottes de Menton," *L'Anthr.* 1898, ix. p. 26.

² E. Piette, *L'Anthr.* 1895, vi. p. 137. See also Moriz Hoernes, *Der Diluviale Mensch in Europa*, 1903, Brunswick. For the latest discovery of a seatoppygous figurine, found near Vienna, see G. G. Maccurdy, *Am. Anthropol.* vol. x. p. 643, 1909.

Enfants near Mentoni has served at various times as a sepulchre; the corpses have not been inhumed but simply laid upon the floor as it existed at the time, and protected by blocks of stone. The earliest of these interments took place in Solutrian times; it was that of an old woman and a boy not more than seventeen years of age; cave earth subsequently accumulated and covered them up, other interments followed, and a continual accumulation of the cave earth, so that when the skeletons of the woman and boy were discovered they lay 10 meters below the surface of the ground. They have been studied in great detail by Dr. Verneau.¹

The skulls are dolichocephalic (index close on 69), the glabella is slightly prominent, the nose flat, with nasal gutters at the base (a peculiarly Negroid character), the jaws prognathous, the chin slightly retreating, the palate parabolic in outline, the teeth large and Australoid in character. The height of the woman was about 1,580 mm., of the boy about 1,540 mm. The average height of the Bushmen is 1,530 mm., with a range between 1,400 and 1,650 mm. This interment proves, therefore, the existence of two individuals belonging to a Negroid race of low stature, and of sufficient consideration to receive a ceremonial burial. We may infer that they lived among friends, and most probably therefore with people of their own race. Steatopygous figurines found in adjacent caves at Mentoni strengthen this conclusion, and the evidence that Mentoni was inhabited in Solutrian times by a race closely allied to the Bushmen amounts almost to positive proof. That this race extended over the south of France and the north of Spain is highly probable. It may even have reached Belgium, as is suggested by the figurine found at Pont-à-Lesse, or Moravia, though we do not as yet know what are the characters of the statuettes found at Predmost.²

That Solutrian Europe was inhabited exclusively by a Negroid race seems unlikely; in this connection the figurines assigned by Piette to another group must not be overlooked. Venturing more deeply into hypothesis, we might suppose that the Negroid Solutrian stock was of Mediterranean origin, and that it pushed its way into Europe till it met with the resistance of an alien population. After a time, under pressure

¹ R. Verneau, "Les grottes de Grimaldi," *L'Anthropologie*, 1906, xvii. p. 291.

² We now know that this Negroid race extended as far as Vienna.



FIG 8—Full length Profile of a Bushman, showing Steatopygia. From a photograph lent by Prof. A. C. Haddon, F.R.S.



FIG 9—Bushman. From a photograph lent by Prof. A. C. Haddon, F.R.S.

from this population, it was forced southwards and finally driven out of Europe.

The Negroid race of pre-dynastic age which has left steatopygous figures of baked earthenware in the Thebaid may be one offshoot from it. Its direct descendants, who retained most fully its culture, habits, and disposition, were the Bushmen as we first knew them.

The Bushmen thus acquire a very peculiar interest for us, and we may therefore conclude with a short account of them, though here again we have to lament many deficiencies in our knowledge, a vast amount of precious information having been irretrievably lost owing to the indifference of civilised governments, and of so-called civilised people, to the history and welfare of the primitive races with which they have been brought into contact.¹

The physical features of the Bushmen may be gathered from the accompanying photographs (figs. 8 and 9), which I owe to the kindness of Prof. Haddon. The habit of the hair to grow curled in pellet-like tufts is well shown in fig. 9.

The Bushman was pre-eminently a hunter. His hunting-ground, which up to the time of the advent of the white man included a large part of South Africa, abounded in game: gemsbucks, gnus, elands, antelopes, giraffes, bison, elephants, rhinoceroses, quaggas, zebras, ostriches, and the wild boar afforded him a rich booty. The weapon he depended on most, both in the chase and war, was the bow and arrow: the bow usually short and the arrows small, but deadly in their effects, since they were invariably poisoned. Different kinds of poison were used, some stronger, some weaker, according to the size and vitality of the intended victim. Scorpions and centipedes ground up into powder and mixed with the poisonous juice of the amaryllis provided one kind; another was prepared from the trap-door spider, a creature of such venom that its bite is said to kill a frog in less than a minute; but the most fatal of all was obtained from the N'Gwa, a little caterpillar about half an

¹ The most comprehensive work we possess on the subject is by G. W. Stow, *The Native Races of South Africa*, London, 1905. A graphic and at the same time scientific account of the last poverty-stricken remnants of the race is given by Prof. S. Passarge, *Die Buschmänner der Kalahari*, Berlin, 1907. See also J. T. Bent, *The Ruined Cities of Mashonaland*, London, 1892; F. C. Selous, *Travels and Adventures in South-east Africa*, London, 1893; and C. Warren, *On the Veldt in the 'Seventies*, London, 1902.

inch in length, the entrails of which furnished a poison so rapid in action that it was employed in hunting the lion. The strength of these poisons is said, however, to vary very considerably; sometimes, like wines, with the year; sometimes with the weather. The poison, when ready for use, resembled a mass of brown or black wax. It was carried in a skin pouch and applied to the arrow by means of a poison-stone, a smooth flat pebble with a deep groove down the middle to hold the poison. This stone was one of the most precious possessions of the Bushman; it is said he would die rather than part with it. The shaft of the arrow was a slender reed, its head a pointed bone, often the leg-bone of an ostrich, which was ground straight and smooth by a grooved piece of sandstone. It was made to fit the shaft, and firmly bound on with animal sinew. A strip of quill was sometimes attached as a barb, and as far down as this the head was carefully covered with poison. Passarge describes the head as consisting of two parts: the pointed poisoned extremity is sheathed point downwards in the basal part till it is required for use. This poisoned extremity was sometimes replaced by a flake of chalcedony or other hard stone, and in later times by iron or bottle-glass.

The arrows were carried in a quiver, furnished with a lid to prevent accidents, though a self-inflicted wound might in many cases have caused no more than a passing inconvenience, since the hunters were acquainted with appropriate antidotes; one of these was prepared from the tuber of a plant, the 'Kalahetlue, which grows wherever the poison grub 'Tha is found. They also possessed antidotes of sufficient efficacy to protect them even against snake bites.

Armed with bow and arrows the Bushmen laid the whole animal world under contribution, and defended themselves from their foes, whether lion or Kaffir, with equal courage and success. In approaching their quarry they were practised in all kinds of cunning. Disguise was a common device; sometimes with a bundle of grass tied on over the head they would glide by fits and starts through the grass so imperceptibly that the feeding herd had no suspicion of their presence. In stalking the wary quagga, the hunter disguised himself as an ostrich, simulated its gait, stopping every now and again to peck and feed, till he approached near enough to let fly his poisoned arrow. For large game they set traps, digging carefully con-

cealed pitfalls, or suspending a heavily weighted weapon over the path to the water pools.

The pursuit of large game was the occupation of the men, but there was also a chase of small game, and this, as well as the collection of vegetable food, was the work of the women. No one who has travelled over the Karoo can have seen without surprise the monstrous ant-hills which disturb the irregularity of the plain: the "eggs" of the ants, or more properly termites, known to the white man as Bushman's rice, were a food they could fall back upon when other resources failed. Provided with a digging stick—that is, a stick pointed at one end and weighted by a perforated stone at the other—the women would unearth large quantities of these "eggs." When sufficient had been obtained they were cleaned by sifting away the accompanying sand, and then, with the addition of a little fat, roasted over a fire until they turned a nice brown. Cooked in this way they are said to have been delicious eating.

Locusts were a favourite dish, and the swarms of these great insects which darken the air in their flight¹ were looked forward to as bringing a time of plenty. They were not only eaten fresh, but preserved for hard times by drying and pounding up into a powder. This was boiled into a sort of porridge, or mixed with honey and made into a cake: in the latter form it was appreciated even by Europeans. Frogs and serpents were dainty eating: poisonous serpents were decapitated before being cooked; their flesh has the flavour of chicken.

The vegetable kingdom was ransacked for all that it could afford, even the seeds of wild grasses were collected and stored for winter use. How short a step it seems from this to agriculture; but to take this step requires qualities that the Bushman never possessed, and inconsistent with his unconquerable love of a wild life. A kind of bread was made out of the pithy interior of *Zamia*, or of the root of *Testudinaria elephas*. Water was carried either in ostrich eggs, sometimes elaborately adorned with incised lines, or in part of the intestine of a zebra or the paunch of agnu. It is pleasant to find that these hardy hunters were not unacquainted with cheerful stimulants; they brewed an excellent mead from wild

¹ I once mistook them for smoke pouring in clouds from a forest supposed to be on fire.

honey, and for tobacco they substituted hemp, which is said to be potent smoking. After a good day's sport they held a feast and spent the rest of the evening in dancing and singing.

The Bushmen were an eminently artistic people. We have already admired their paintings, but, like Solutrian man, they also engraved animal figures on the rocks, not, however, by incised lines, but by punching holes, so that the resulting line was "dotted."¹ A case has been recorded, however, of an antelope graven in relief, and another actually drawn with incised lines.² It has been asserted that the Bushmen painters and the Bushmen sculptors belonged to different branches of the race, but this is open to doubt.

They did not go altogether without clothing, and on state occasions dressed themselves in rich mantles of fur. Great care was expended on the preparation of these; the skin was first cleaned of all fat and superfluous material by scraping with a flint implement, it was then rendered soft and supple by stretching, rubbing between the hands and trampling with the feet. Both sexes adorned themselves with beads made from fragments of ostrich-shell; the shell was broken into little angular pieces, a hole was drilled in each piece with a flint borer, and then the corners were rounded off and the edges smoothed away. Necklaces of these white beads look well against the warm brown tint of the skin. Certain districts possessed a reputation for making these beads, which were a regular article of commerce.

The Bushmen were intensely fond of music, and had made greater advances in this art than any of the other races of South Africa; appropriate music and song accompanied each of their numerous dances. Of their musical instruments especial mention may be made of the four-stringed harp which had been evolved out of the bow, and of a combination of twelve bows which formed a primitive dulcimer.

We may recur for a moment to the Bushman's paintings in

¹ I shall not readily forget the surprise with which I came upon the figure of an antelope outlined on the surface of a *roche moutonnée* which was glaciated during the upper Carboniferous epoch: this was near Riverton, on the Vaal; Stow mentions a finely sculptured eland in the same locality.

² L. Peringuey, "On Rock-gravings of Animals and the Human Form, etc.," *Trans. South African Phil. Soc.* 1906, xvi. It may be noted that some of the Solutrian drawings are *pointillés*.

order to point out a fact of considerable importance in connection with the cave paintings of Europe. We are told¹ on the authority of the Bushmen themselves that it was not any man of a tribe who was competent to make a painting: it was only those who were specially gifted, and when an artist had adorned the walls of a cave with his polychromes no one would dare to interfere with them so long as he was alive, nor indeed so long as his memory lasted. It was only when his name had passed into oblivion that a new aspirant for fame would venture to make fresh drawings over the old ones. In some caves as many as five distinct series of paintings are to be seen one over the other.

As regards the interpretation of the paintings, Stow does not attribute a mythical meaning to them, but asserts that they are historical, though he admits that a mythical meaning may have subsequently become attached to them. In no case do they appear to have been concealed from women and the uninitiated.

A certain amount of government had been established among the Bushmen; there were head chiefs to the tribes and sub-chiefs to the families or clans; the hunting-grounds of each family were strictly delimited, and the boundaries were faithfully observed. It is said that the head chiefs had their residence in great caves, and that the paintings in these were the emblazonment of the symbol of the tribe.

All that we learn about the Bushmen impresses us with their great intellectual ability. Johnston mentions one individual he met, who conversed fluently in Dutch, spoke more English than many Boers, and was thoroughly conversant with Hottentot, Ochi-herrero, Ochi-mpo, and several Bantu dialects.² They were distinguished for their hospitality to strangers, and for the unselfish way in which they divided their food. They loved their country and showed an unfailing devotion to their chiefs; they possessed all the noblest of the primitive virtues, and, not least, unflinching bravery and unquenchable love of freedom. It was this last which came to be accounted to them as their greatest crime. They found it impossible to become slaves to strange masters in their own land. Equally impossible was it for a hunting race to maintain

¹ Stow, *op. cit.* p. 26.

² Johnston, *Tribes of the Congo*, Mem. Anthr. Inst. 1884, xiii.

its existence in proximity to an encroaching agricultural people of European blood. A terrible war of extermination was waged against them by the Boers. The stories that are told of this are shocking to our humanity; and we cannot refuse a tribute of admiration to these brave people, who in almost every instance preferred death to surrender. Almost the only exception recorded is that of a chief who, surrounded by foes, replied to repeated calls to yield by arrows from his bow; at length, as these ran short, he accepted quarter and delivered himself up, whereupon his brains were immediately blown out. The last to be killed in this war was one of the painters. Upon his body there was found a leathern belt with twelve little horns strung to it, each containing a different pigment.

We have spoken of the Bushmen in the past tense, for their nation is destroyed; a miserable remnant, however, lingers on in the Kalahari desert, but these are slowly dwindling away under the terrible hardships of an unfavourable environment.

As we have seen, the Bushmen when we first knew them inhabited the southernmost part of Africa, while Solutrian man occupied at least a part of Europe. If then the European Solutrians were the ancestors of the Bushmen, they must have traversed the whole length of Africa before arriving at the Cape; and Stow, who possessed an unrivalled knowledge of the Bushmen, was led by independent investigation to conclude that the Bushmen must have migrated from the north, southwards; he has even gone so far as to indicate their route. One branch of the race kept more to the westward side of the continent in their journey south, the other kept more to the east. Stow asserted that the western branch were the painters, the eastern the sculptors or engravers, and that where they came in contact the two arts were intermingled, precisely as in Solutrian Europe (fig. 10). But doubts have been expressed as to the truth of this generalisation.

This wonderful little people, who with an average of only 1,330 cc. of brain managed to accomplish so many great things, is now practically extinct, but they leave behind an imperishable memory; they have immortalised themselves in their art.

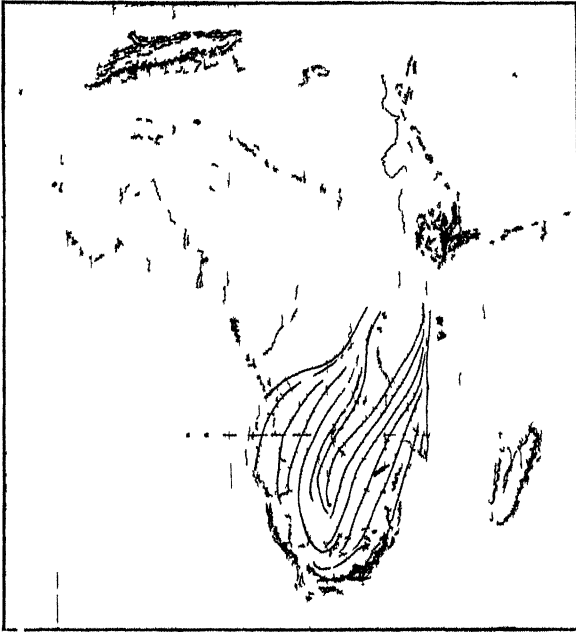


FIG. 10 Map of the Gradual Migration southward of
the Bushmen After Stow

THE PROBLEM OF THE PYCNOGONS

By W. T. CALMAN, D.Sc.

THE small group of marine animals known to zoologists as Pycnogonida, Pantopoda, or Podosomata, and by the more popular but somewhat misleading names of "Sea Spiders" or "Nobody Crabs," have always been a stumbling-block to those who have attempted to draw up a natural system of classification for the Arthropoda. They have been ranked sometimes with the Arachnida and sometimes with the Crustacea, they have been placed by themselves as a separate division of the Arthropoda,¹ and one investigator has even attempted to demonstrate their affinities with the Myriopoda. The recent discovery, in Antarctic Seas, of two surprisingly aberrant members of the group, instead of throwing light on their systematic relations, has only added to the difficulty of interpreting them.

A typical Pycnogon, such as *Nymphon*, has a slender body composed of a cephalic segment or "head," followed by three free body-segments (somites) and a small terminal lobe known as the abdomen. Four pairs of disproportionately long legs are attached, the first pair to the cephalic segment and the others to the three following body-segments. In addition, the cephalic segment bears three pairs of appendages, the first being chelate (pincer-like) and overhanging a long tubular proboscis which bears the opening of the mouth; the second pair are sensory "palps" placed at the sides of the proboscis; and the third pair, immediately behind these, have, in the male sex, the function of carrying the eggs deposited on them by the female, and are therefore known as "ovigerous legs" or "ovigers."

It is at once apparent that there can be no close comparison between the plan of structure here outlined and that of any

¹ For the benefit of those unfamiliar with zoological classification it may be explained that the comprehensive group Arthropoda includes the classes Crustacea (crabs, lobsters, shrimps, etc.), Arachnida (scorpions, spiders, king-crabs, etc.), Diplopoda (millipedes), Chilopoda (centipedes), Hexapoda (insects), and Onychophora (*Peripatus*). The Diplopoda and Chilopoda are sometimes regarded as forming a single class, Myriopoda.

of the Crustacea. The typical members of that class have two pairs of appendages in front of the mouth (antennules and antennæ), and at least three pairs of post-oral appendages of which the basal segments act as jaws (mandibles, maxillulæ and maxillæ). The details of the appendages and the structure of the internal organs give no evidence in favour of an alliance between the Pycnogonida and the Crustacea. A peculiar larval stage, common to many Pycnogons, in which only three pairs of appendages are present, has been compared with the nauplius-larva of Crustacea, but the resemblance hardly extends beyond the agreement in the number of the limbs. The chelate form of the first pair and the uniramous character of the second and third, as well as the presence of a suctorial proboscis, are important points of divergence from the nauplius type.

The general resemblance to an Arachnid which led Ström, in 1762, to give the name *Phalangium marinum* to the first described Pycnogon, is due chiefly to the four pairs of long and slender legs. It is increased by the chelate form of the single pair of appendages which lie in front of (or above) the mouth, and correspond to the cheliceræ of the Arachnida. Beyond this, however, the comparison of the appendages becomes involved in difficulties, for the Arachnids possess but one pair of appendages (the pedipalps) between the cheliceræ and the first pair of legs, while in the Pycnogons two pairs, the palps and the ovigers, occupy the same position.

Some zoologists who appear to estimate the relative value of facts according to the difficulties in the way of their investigation, attach greater importance to the internal anatomy of the Arthropoda than to their external characters, and to the remoter details of their embryology than to the general plan of structure of the adult animals. While evidence from all these sources must be allowed due weight in attempting to decide as to the systematic affinities of organisms, it seems clear that, in the present case, parallels between the Pycnogonida and Arachnida in the hepatic diverticula of the alimentary canal and in the mode of formation of the germ-layers in the embryo can hardly be brought into the argument while we are faced by a serious discrepancy in the comparison of the somites and appendages of the two groups.

Attempts to minimise or to explain this discrepancy have not been wanting, but only two of these can be mentioned here.

According to one view the ovigers of the Pycnogonida are homologous with the first pair of legs in the Arachnida, while the last pair of legs in the former group represent the first of the abdominal (mesosomatic) appendages of Arachnida, or, according to Lankester and Pocock, those of the pregenital somite which is without appendages in all existing Arachnida. On this view the coincidence in the number of the walking-legs in the two groups loses the significance attributed to it by the older observers. A more serious objection is that the distinction between prosomatic and mesosomatic appendages is sharply marked in all Arachnida known to us from Silurian times down to the present. It may be laid down as a general rule that when, in any division of the Arthropoda, a grouping of the appendages into distinct series or "tagmata" becomes well established, it is rarely, if ever, completely obliterated in the further evolution of the group. For example, among Crustacea, the grouping of the trunk-limbs into the two tagmata of the thorax and abdomen, which is one of the most distinctive features of the sub-class Malacostraca, never becomes lost amid the varied modifications which these limbs undergo. Accordingly, the absolute identity in structure between the last and the preceding pairs of legs in Pycnogonida would seem to imply, on this view of their homologies, that the delimitation of the prosomatic and mesosomatic regions had not been established when the Pycnogonida diverged from the main Arachnid stock, which takes us a very long way back indeed.

Another view of the homologies of the Pycnogonid limbs is that advocated by Carpenter, and is based on an observation by Lendl, who states that, in the development of a spider, a pair of transitory rudiments of appendages make their appearance between the chelicerae and pedipalpi. There is thus provided an equivalent for the palpi of the Pycnogonida, and we are permitted to retain the obvious and traditional comparison between the eight legs of the "Sea-Spiders" and those of the more normal Arachnida. The only objection to accepting this as a satisfactory solution of the problem is that Lendl's observation stands, as yet, quite alone. His account is, indeed, clear and detailed, but it is not accompanied by figures, and none of the other investigators of Arachnid embryology appears to have seen the rudiments which he describes. It is much to be desired that the subject should be reinvestigated.

One of the most important and unexpected biological results of the recent explorations in Antarctic seas has been the discovery of ten-legged Pycnogons. The first of these was described by Mr. T. V. Hodgson, shortly after the return of the "Discovery" expedition, under the name *Pentanympyon antarcticum*. As a matter of fact, however, a decapodous Pycnogon had been described so long ago as 1837 by Eights, but his description had remained unknown to the majority of naturalists, while those who knew of it either misinterpreted his account or set it aside as erroneous. Shortly after describing *Pentanympyon*, Mr. Hodgson had the good fortune to discover, among the collections of the Scottish National Antarctic Expedition, the long-lost *Decolopoda australis* of Eights, and his redescription of it was published almost at the moment when another naturalist was confidently asserting the impossibility of its existence.

The possession by some Pycnogonida of five, instead of the usual four pairs of legs necessitates a reconsideration of our views as to their morphology and affinities, and it is hardly too much to suppose that, if the ten-legged species had chanced to be discovered before those with eight legs, the Arachnid view of their affinities would not have received so early or so general acceptance.

At first sight *Pentanympyon* and *Decolopoda* would seem to give support to the theory of Lankester and Pocock outlined above. Having once, as it were, encroached upon the mesosomatic region of the body in searching for homologies for the Pycnogon legs, the necessity for taking two pairs instead of one pair from this region presents no additional difficulty. To Carpenter's view, on the other hand, the presence of a fifth pair of legs offers a considerable obstacle, and destroys the simplicity of the comparison between Pycnogons and Arachnids.

When we come to examine more closely the relations in which the ten-legged species stand to the other Pycnogons, fresh difficulties arise. It might have been expected that two forms presenting so important a divergence from the normal structure of the group would prove to be closely related to each other. This, however, is by no means the case. *Pentanympyon* differs in no respect, save in possessing an additional pair of legs, from the genus *Nympyon*, which is in many respects

one of the most central and unspecialised genera of the group; but *Decolopoda* departs widely from this type, and approximates to the very different *Colossendeis*. Two explanations of this state of things are possible. The first and most obvious is that the ten-legged condition is a primitive character which has been retained by the most primitive members in each of two diverging branches from the ancestral stock of the Pycnogonida. Something like this view is held by Bouvier, who assigns to the one branch *Decolopoda* and *Colossendeis* (Colossendeomorpha), and to the other *Pentanymphe* and all the other Pycnogons (Pycnogonomorpha). Bouvier, however, admits that *Pentanymphe* is in some respects (e.g. in the number of segments in the chelicerae and palps) less primitive than some of the other genera assigned to the Pycnogonomorpha, and, as a whole, the scheme of classification which he gives seems forced and unnatural.

Carpenter, faced by the difficulty which the ten-legged species present to his scheme of homology between the Arachnid and Pycnogonid appendages, suggests with some hesitation that "the fifth pair of legs in these genera may possibly represent a comparatively new development," and not a primitive character—implying also, it is to be presumed, that this development has arisen independently in the two cases. It must be admitted that the constancy in the number of somites and appendages throughout the comparatively wide range of structure presented by the eight-legged Pycnogons strongly suggests that this is the deep-seated and, so to speak, "normal" plan of structure of the group; while the fact that the decapodous condition appears in two genera, which have little else in common, is in favour of the view that this is a secondary modification of the original plan.

There is one case among Crustacea, to which attention has not hitherto been drawn in this connection, which appears to bear a certain analogy to that of the ten-legged Pycnogons. The genus *Polyartemia*, belonging to the order Anostraca in the sub-class Branchiopoda, differs from all the other members of the order in having nineteen somites between the head and the "genital segment," which bears the openings of the reproductive organs. In all the other Anostraca there are eleven somites in this region of the body, and the very close resemblance in other respects between *Polyartemia* and the rest of the

Anostraca suggests that the nineteen somites in the one case correspond, as a whole, to the eleven somites in the other. Further, in the other orders of the Branchiopoda (except the Cladocera, which have the body very much shortened) the genital openings are either on the eleventh or on the twelfth trunk-somite, so that there is some ground for believing this to be approximately the primitive position, from which *Polyartemia* alone has departed. In other words, it seems as though the number of pre-genital somites, which in the evolution of the group had become established as a constant character, had become unstable, and reached a new "position of equilibrium" in the case of *Polyartemia*. If we may assume that something analogous has happened in the case of *Decolopoda* and *Pentanymphon*—that they are, like *Polyartemia*, departures from the normal line of evolution of the group to which they belong—then it is permissible to leave them out of account in considering the affinities of the group as a whole.

Huxley wrote long ago that "in the absence of any adequate palæontological history of the Invertebrata, any attempt to construct their Phylogeny must be mere speculation." So far as the great bulk of the Arthropoda, at any rate, are concerned, this adequate palæontological history is still to seek. Nevertheless, some attempt to construct a Phylogeny—speculative though it may remain—must be the basis of any attempt at a natural scheme of classification; and, in spite of the modern tendency to confine biological research to matters which admit of experimental or statistical verification, some approximation to a natural classification is an intellectual necessity for the biologist.

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REVIEWS

The Physics of Earthquake Phenomena. By CARGILL GILSTON KNOTT, D.Sc., F.R.S.E. [Octavo, pp. xii. + 283.] (Oxford: At the Clarendon Press, 1908. 14s. net.)

THE author has several qualifications for his task. During eight years he was Professor of Physics at Tokyo in the Imperial University of Japan, which gave him a first-hand knowledge of the practical side of earthquakes. While resident in Japan, and subsequently, he has made earthquake phenomena the subject of several original researches. Finally, so far as we know, he has not invented a seismograph. His aim, as is mentioned in the preface, is not to give a complete account of all earthquake phenomena, but merely of such as are intimately connected with physical theory. The nature of the work was presumably determined to a considerable extent by the fact that it is the outcome of "Thomson Lectures" delivered in Aberdeen in 1905-6. The audience, though hardly to be described as popular, was not one deeply versed in physical science, and minute details of mathematical investigations would have been out of place. This presumably explains why there is comparatively little mathematics in the book, though it refers to a variety of subjects requiring advanced mathematics for their complete presentation. Those who wish fuller mathematical treatment will have to go elsewhere. At the same time, they will obtain a clearer idea of the true nature of the problems presented, and of the difficulties remaining to be surmounted, than is obtainable from any previous book published in England.

After a brief survey of earthquake phenomena, the author discusses the amplitudes of the earth movements produced, and how the energy of the shock varies with the distance from the epicentre. Following a short description of the Milne, Omori, and some other forms of seismograph, there is a discussion of the fundamentally important problem of forced vibrations and resonance effects in the pendulums which form the essential part of most seismographs. Special consideration is given to the recent work of Galitzin, which deserves to be better known than it is in this country.

A short chapter, based mainly on Milne's work, is devoted to the geographical distribution of earthquake origins. The question of the supposed periodicities—daily, lunar, annual—in the frequency of earthquakes is discussed in considerable detail in two chapters. The author makes use of Schuster's criteria in deciding as to the reality of the various periodicities, and adopts a distinctly critical attitude. Thirty pages are devoted to some aspects of the mathematical theory of elasticity, auxiliary to the study of the propagation of earthquake waves, and to the reflection and refraction of waves at a plane surface separating two different media. Two long chapters deal with seismic records—various specimens of which are reproduced—and with the nature and velocity of the different types of waves to which different portions of each earthquake record are due. The last chapter relates to a number of disconnected problems, including Milne's investigation into the supposed connection between the frequency of large earthquakes and the small movements of the earth's axis.

Whether the author's views on disputed questions will all prove to be correct time alone will show. On some points he certainly seems to form a judgment rather lightly; but as a whole the book is unquestionably well worthy the attention of those who wish a summary of the present state of seismic knowledge and an indication of the principal problems remaining to be solved. Some of the illustrations are excellent, especially those showing records from Milne seismographs and those illustrating Galitzin's experiments; but there are a few—*e.g.* figs. 3, 4, 7, 10, 11, and 15—reduced apparently from large diagrams or maps, in which details are so minute as merely to irritate the eye. Some of the chapters show signs of somewhat hasty revision. The devotion of Prof. Milne to the subject is worthy of all praise, and the author's references to the results of his labours on p. 187 are well deserved; but when the reader finds practically the same remarks repeated on p. 198 he instinctively begins to think of Aristides. In a few cases—*e.g.* in the tables on pp. 203 and 215—the exact nature of the data collected and the units employed might with advantage have been more clearly stated. In other cases—*e.g.* on pp. 165 and 167—in the matter of wave velocities, the cart seems to get a little in advance of the horse. Few actual slips have been noticed. On p. 96 there seems a confusion between North and South Pacific. There is obviously a misprint in the value of Poisson's ratio on p. 165 for "Lily Lake" granite. In several cases—*e.g.* p. 78—quantities are neglected or described as inappreciable when they are only relatively small, and not, strictly speaking, negligible. A further criticism that is not unlikely to be made is that the price seems high for a book of the size. It only remains to add that there is a table of contents, an enumeration of the illustrations, and an index, so that reference to specific points should be easy.

C. CHREE.

General Chemistry for Schools and Colleges. By ALEXANDER SMITH.
[Pp. xiv. + 530 with 65 figures.] (George Bell & Sons, 1908. 6s. 6d. net.)

OF the making of text-books of chemistry there is no end, but in the compilation of the present treatise the author has certainly not travelled along the well-worn track.

In four introductory chapters a few simple experiments are discussed with the object of defining what are the characteristics of chemical changes as distinct from those of other natural phenomena. A detailed consideration of these typical reactions shows that there are six characteristics of chemical change, several of which can only be appreciated by quantitative experiments. The author is thus led at a very early stage to emphasise the importance of exact measurements as the only safe guide to the elucidation of chemical changes.

A few atomic weights are taken for granted in the third chapter in order that symbols, formulae, and equations may be explained; after which follow several chapters on general and descriptive chemistry. In Chap. XII. the author reverts to and discusses more fully the more difficult parts of the subject in connection with the determination of atomic and molecular weights.

Although the intermingling of chapters on descriptive chemistry with those on fundamental principles may have the effect of imparting variety to the student's reading, yet in some instances the arrangement of topics is somewhat arbitrary. For example, a chapter on chemical equilibrium is interposed between one on the halogen family and another on the oxides and oxygen acids of the halogens. The chapter on ozone and hydrogen peroxide is sandwiched between chapters on

dissociation in solution and ionisation. The description of the halogen family usually affords a favourable opportunity for introducing the periodic classification, but this important generalisation is not mentioned until selenium and tellurium are reached on p. 271. The quantitative consideration of ionic equilibrium might well have formed the conclusion of the section on ionic substances and their interactions, instead of being introduced into the chapter on sodium and lithium.

The treatment of each separate section leaves nothing to be desired, the fundamental principles in particular being presented and illustrated unconventionally, but with thoroughness and lucidity. Finally, the student's interest cannot fail to be stimulated by the searching questions which are asked at the end of nearly every chapter.

G. T. MORGAN.

A Plant Book for Schools. By OTTO V. DARBISHIRE, B.A. (Oxon.), Ph.D. (Kiel). [Pp. vi. + 164, with 115 illustrations, mostly from photographs.] (London : Adam & Charles Black. 2s. 6d.)

THERE can be nothing but praise for this little book. It has been designed as an easy introduction to the study of plant life for the use of young children, the author having especially before his mind the case of those children who are being taught at home by their parents. The well-chosen subject-matter and the direct, often quaint, simplicity of language have resulted in the production of a book which should awaken in the minds of young readers a very real and sustained interest in the plant world around them : and it may be safely said that the book is one which all those who are entrusted with the teaching of young people might read and mark with much advantage to themselves and to their charges.

The first four chapters serve as a general introduction. They deal with the different members of the plant body, and present to the mind of the student a conception of the plant as a living organism actively engaged in the performance of vital functions. The subsequent chapters are essentially an elaboration of the introduction together with a broad survey of the principal divisions of the vegetable kingdom. The author boldly—and, we think, successfully—introduces to his young readers, in their very First Book, a fern, a moss, an alga, and a fungus, in addition to flowering plants ; even the fossils are not neglected.

The method of teaching advocated by the author is one which now finds almost universal acceptance, viz. one in which insistence is made upon the necessity for each student having, actually in his hands, living specimens of the plants under discussion. This, indeed, has been made the feature of the book under notice. The volume is, strictly speaking, a simple text-book ; but it is one in the use of which the student is expected to verify, by comparison with actual specimens, or by direct experiment, practically every statement made in it. In other words, the author has presented a course of study in which there is no distinction drawn between all that is conveyed in the formal terms of "lecture" and "practical work."

The concluding chapter contains information of a practical character with regard to the obtaining of specimens, the provision of apparatus and the making of botanical collections. The remarks should prove useful to teachers and parents, but it is curious to learn that, for the purpose of drying plants, "blotting-paper is useless." The material is not perfect, but, surely, countless thousands of herbarium specimens bear silent testimony against the author.

S. E. CHANDLER.

Types of Floral Mechanism. By ARTHUR H. CHURCH, M.A., D.Sc. Part i. Types i.-xii. (January to April). [Pp. vii. + 211.] (Oxford : at the Clarendon Press, 1908. £1 1s. net.)

THE Oxford University Press, to which botanists for many years have been very deeply indebted for its enterprise in publishing, amongst other volumes, the well-known series of scholarly translations of botanical classics, has earned their further gratitude by issuing this, without doubt one of the most interesting botanical books of recent years, and one which in its turn should become a classic. On first turning over the pages one is filled with admiration of a great work of art. The size, royal quarto, the beauty and close adherence to nature of the thirty-nine coloured plates, the delicacy and accuracy of the very numerous other plates and illustrations in the text, the clearness of the typography, all combine to render the mere casual examination a deep and lasting pleasure. To the better type of student the book must serve as a valuable stimulus, although the modest remark of the author in his introduction that "all figures are drawn in the size and style and of the character to be expected of good-class student work" might well inculcate a feeling akin to despair in those not naturally endowed with the gift of draughtsmanship so richly bestowed upon Dr. Church.

The aim and scope of the book are sufficiently indicated in the sub-title : "A selection of diagrams and descriptions of common flowers arranged as an introduction to the systematic study of Angiosperms." Twelve types are nominally dealt with, all spring flowers, but comparisons with allied forms are introduced under each type, so that the actual number of plants described and discussed in this part is actually but two short of fifty.

An examination in more detail of the mode of treatment of one type, e.g. *Viola odorata* and its congeners (pp. 88-117), will serve to indicate the wealth of information the volume contains. After briefly summarising the history and geographical distribution of the main type, a detailed description of the inflorescence and flower is followed by notes on the variations ordinarily met with, and students find food for thought and for further observations of their own on other plants in the incidental note that "being a highly specialised floral mechanism, the flower of the violet is remarkably constant under cultivation. Improved garden forms and florists' varieties differ only in the larger size of the plant parts as a whole and of the petals of the flower, the essential organs remaining practically unaffected." The floral diagram and sectional elevation planned on a comparable scale serve to elucidate the primary construction of the adult flower and are supplemented by a detailed study of its developmental history, the illustrative figures being those obtainable by the use of a "low power." The development of the special mechanism of the flower is similarly traced, and incidentally interesting notes such as the use of "syrup of violet" paper as a test for acids and alkalies previous to the introduction of litmus are given. Pollination, with notes on the habits of various insect visitors and their comparative dexterity, a good account of cleistogamy, monstrosities, and the fruit and seed complete this very full survey of the type. The allied forms *Viola tricolor* var. *arvensis* (the corn pansy) and *Viola altiva* (the garden pansy) are next fully dealt with, and the section concludes with a discussion of theoretical considerations which arise out of a study of the genus *Viola* in relation to other types and to other members of the same natural order. This general discussion will be of great interest and value to students apt, quite naturally, too often to regard natural orders as sharply delimited divisions, by the author pointing out that the very existence of the order *Violaceæ* is probably due to the wide distribu-

ion in temperate regions of the specialised genus *Viola* which became well known to the early botanists, and was made a type around which tropical genera were subsequently segregated. The tropical genera of *Violaceae* are, however, simpler, and with the genus *Viola* lacking they might well have been merged in another order, e.g. *Flacourtiaceae*. That is to say, "in this case as in many others our view of the grouping of the plant kingdom is largely influenced by European bias."

Enough has been said to indicate the character and the original and refreshing mode of treatment adopted in this remarkable book. It is one which all teachers of botany and all lovers of flowers would be glad to possess and one which, properly used, cannot fail to do a great deal to elevate the standard of botanical teaching. Dr. Church and the Oxford University Press are indeed to be congratulated on the respective parts they have played in the production of this magnificent work.

W. G. FREEMAN.

Insectivorous Plants. By CHARLES DARWIN, M.A., F.R.S. Revised by FRANCIS DARWIN. [Pp. xiv. + 377.] (London: John Murray, 1908. 2s. 6d. net.)

WITH the publication of this volume Mr. Murray has brought us another step nearer to being able to obtain the whole of the works of Charles Darwin at the price of 2s. 6d. each. We trust that he has set himself to the completion of this task, and that, ere long, even the present few absentees will be numbered amongst the members of this "Popular Edition." *Insectivorous Plants* was originally finished in 1875, some sixteen years after the author made his first observations on the subject. It affords an excellent example of Darwin's extraordinary power of making extremely delicate observations with very simple apparatus, and in the past has often roused admiration and enthusiasm in young naturalists. It is of interest to recall Darwin's comments to Sir J. D. Hooker on the book just before its completion (*Life and Letters*, vol. iii. p. 328): "You ask about my book, and all that I can say is that I am ready to commit suicide; I thought it was decently written, but find so much wants re-writing, that it will not be ready to go to the printers for two months, and will then make a confoundedly big book. Murray will say it is no use publishing in the middle of summer, so I do not know what will be the upshot; but I begin to think that every one who publishes a book is a fool." The first edition, however, was sold out almost at once; and a second edition was issued and again re-printed in the following month. Three further reprints followed, and now we have this popular edition, an exact copy of the earlier more expensive issues, bringing this classic within the reach of all.

The Cell as the Unit of Life, and other Lectures. By the late ALLEN MACFADYEN, M.D., B.Sc. Edited by R. TANNER HEWLETT, M.D., F.R.C.P., D.P.H. [Pp. xvi. + 381.] (London: J. & A. Churchill. 7s. 6d. net.)

THIS volume contains several series of lectures delivered by the late Dr. Macfadyen during his tenure of the Fullerian Professorship at the Royal Institution. The first series of five lectures deals with the morphology, chemical composition, and general physiological action of the cell, and the second series deals more especially with the metabolism, and the characters of the intracellular enzymes. The third series supplements the two previous courses of lectures, giving some account of the more recent methods of investigation and of technique, including the author's own

process for the preparation of intracellular products. A fourth series of lectures is devoted entirely to the toxins and antitoxins.

The subjects chosen by the author have been introduced and treated in an interesting manner, but criticism in detail is somewhat disarmed by the statement of the editor, that he has hesitated to make alterations either in the matter or the wording unless obliged. As the first series of lectures was delivered as long ago as February 1901, many of the statements contained therein can scarcely be taken to represent quite modern views on the subjects treated. This remark refers more especially to the account given of the chemistry of the cell and the composition of the protein constituents.

Other statements require, again, amplification. Thus, for example, on p. 89 it is stated that α -glucoside is attacked by the ferment of yeast, whereas the β -glucoside is left intact. To the general reader without special chemical knowledge, to whom this book should appeal, the terms α - and β -glucoside are meaningless. No doubt, in the course of the lectures, an explanation was added which did not appear in the manuscript, and the book would have been of greater value had such additional explanations been added in the form of footnotes.

A large part of the lectures is taken up with a description of fermentative processes, and the account given of the various enzymes and their action is, for the most part, admirable. The book, as a whole, can be recommended to the general reader who wishes to follow some of the more modern developments of biology, especially that part which relates to the investigation of disease.

S. B. SCHRYVER.

Animal Life. By F. W. GAMBLE. [Pp. xvi. + 305, with 63 illustrations.] (Smith, Elder & Co., 1908. 5s.)

DR. GAMBLE'S "excuse" for writing this book is "the want of a small work dealing with the adaptations and factors of animal life in a broad and connected manner." We can assure him that no apology is needed; there is no other work in our language that covers quite the same ground, or that gives so wide a survey of the problems of animal life in the same compass. In the opening chapters are discussed the contrast between animal and plant life, the fulness of the earth, and the general organisation and classification of animals. The succeeding pages are devoted to the mechanical problems of movement, the search for food, the demand for oxygen, and the response and orderly adaptation to surroundings. The eighth chapter is perhaps the most remarkable in the book, for here the author, in dealing with the colours of animals, has introduced work which has not previously found its way into books readily accessible. It would be hardly fair to give away the secret of this chapter, but we strongly advise all who are interested in animal coloration to read what Dr. Gamble has to say concerning the evolution of red and yellow fatty pigments, and the association of these colouring matters with stores of reserve food. The influence of light upon the activity of these pigments is most interesting and suggestive of further research along similar lines.

The two last chapters are concerned with the welfare of the race and its bearing on the individual, and with the life-histories of insects. In dealing with these last the author allows himself to enter into fuller detail of structure than he has elsewhere adopted; and many of the problems of nutrition and respiration which we had missed in the chapters assigned to these topics are relegated to this section. The arrangement thus selected is probably the wisest, and much repetition is thereby avoided. It is hardly necessary to state that the keynote throughout is

evolution and adaptation. We have detected very few errors of any sort: on p. 88 there is a wrong reference where "fig. 12" should be "fig. 14"; on p. 250, ll. 2-7 is a passage that is not very clear, and seems to have got out of place; while it is not strictly correct to speak, p. 257, of the "sting" inflicted by Owl-midges.

The book is one to be enjoyed alike by the general educated public and by trained zoologists. It is written in a delightful style and contains many picturesque and even eloquent passages which will bear reading many times over. It is printed in excellent type and has on the cover a most graceful, though simple, design, that is perhaps intended as a symbol of the bird's-eye view of creation displayed within.

O. II. LATTER.

Internationale Revue der gesamten Hydrobiologie und Hydrographie.

Band i. Heft 1-2, May 1908. (Klinkhardt, Leipzig. 12 Mark.)

WE welcome most cordially the appearance of a new Journal devoted to hydro-biological and hydrographical studies. During the last ten years the synthesis between oceanographical and biological research has been effected, and both kinds of investigation are now carried on concurrently, with results of very great theoretical interest as well as practical importance. But, hitherto, papers dealing with such work have been published in all kinds of periodicals—Blue-books, geographical journals, fisheries reports, etc., and this scattered condition of the literature has been a decided drawback, for it has become very difficult for the student of these matters to keep in touch with recent literature. The new Revue deals with Oceanographical Investigations, Limnology, Planktology, and the biology of marine and fresh-water basins so far as this is to be associated with the study of the chemical and physical conditions obtaining in those media. It will therefore supply a want which must have been experienced by those engaged in these investigations.

The combined Heft now before us contains a "Foreword" by Prof. Weismann, and, most appropriately, an introductory article by Sir John Murray dealing with those chemical and physical conditions in the hydrosphere which are likely to affect the distribution and abundance of organisms. Richard Hertwig writes on the opportunities for investigations of a fundamental nature which would be afforded by the establishment of fresh-water biological stations. Raffaele Issel contributes a short article dealing with thermal biology, and referring specially to the fauna and flora of certain hot springs in Piedmont. A series of contributions to the biology of the plankton of the sea begins with a long article by Alexander Nathansohn on the general conditions of productivity in sea areas. Alfred Fischel discusses methods of vital staining, with special reference to fresh-water Cladocera. C. Klausener and G. Götzinger give accounts of the physical characteristics and faunas of certain lakes in Switzerland and Austria.

In addition to these original contributions the Heft contains a number of general accounts of the results of investigations which have been published *in extenso* elsewhere. The physical and biological investigation of fresh-water lake basins receives considerable attention: thus there are two summaries of the work of the Scottish Lake Survey, and summaries of work carried out in lakes in Wisconsin, Ceylon, the higher Alps, and in Lake Victoria Nyanza. There is also a general account of the progress of marine exploration in Austrian seas, and of recent investigations with reference to the migrations of food-fishes in the North Sea and the Baltic.

A long series of reviews of biological and hydrographical literature, and a number of notices relative to the organisation and work of biological stations and institutes, completes the contents of the *Revue*. We are sure that this publication will be of great service to those engaged in hydrobiological research, and that it will, as Sir John Murray remarks in his introductory article, "contribute greatly towards the elucidation of the many unsolved oceanographical problems."

J. JOHNSTONE.

Heredity. By J. ARTHUR THOMSON. [Pp. xvi. + 605.] (London: John Murray, 1908. 9s. net.)

THIS is certainly the best modern book on heredity to recommend to the student and to the intelligently curious. It is, in fact, the only book on heredity which is not controversial. Mr. Punnett's "Mendelism" is, as frankly announced in the title, devoted strictly to Mendelism. Mr. Lock's "Variation Heredity and Evolution" deals almost exclusively with Mendelism; other topics are dealt with, but not with sympathy, especially in the case of biometry. Mr. Bateson's researches have made his "Mendel's Principles" out of date. The numerous papers on heredity which have appeared in the pages of *Biometrika* are consistently antagonistic to Mendelism. And in Dr. Archdall Reid's "Principles of Heredity," a great and constructive work, very little sympathy with either of these schools will be found. Mendel's idea is proclaimed by his adherents as the most valuable tool which has so far been placed in the hands of the breeder of animals and plants: yet in De Vries' "Plant Breeding" no reference to Mendelian work is to be found. Prof. Thomson's book gives a comprehensive and readable account of the main facts of heredity, and a critical but unbiassed account of the more important theories which have been put forward to account for them. The author has looked on well-nigh every aspect of this vast subject, and, having no gospel to preach, has been very just in the amount of space which he has devoted to the various sections. And the expectation, based on Prof. Thomson's previous works, that this book will be clearly and forcibly written is fulfilled. We could have no better guarantee that Prof. Thomson's criticism is unbiassed than the fact that he does not belong to any of those modern schools of biological thought whose mutual antagonism has done so much to clear the air and to reveal the real nature of the problems which we have to solve. But we do not wish to convey the impression that our author is a mere mechanical compiler; on the contrary, current notions are analysed and current misunderstandings are exposed with a degree of penetration which can only be the result of much careful deliberation and thought. His attitude to the more fundamental aspects of the question appears to us to be very sound. If he errs at all, he errs in the direction of excessive caution; but, in our opinion, at the present moment this is not an error; and we hope it will serve to counteract that lack of self-mistrust which characterises too many of our modern investigators of natural processes. We heartily sympathise with the philosophy which prompts the statement "that to speak of the 'Principle of Heredity' in organisms is like speaking of the 'Principle of Horology in clocks.' And we are firmly convinced that the sooner we get rid of such verbiage the better for clear thinking, since heredity is certainly no power, or force, or principle, but a convenient term for the relation of organic or genetic continuity which binds generation to generation."

Omissions in a book of this small compass there must be, of course, as the author frankly regrets to be the case. But considering that such widely different topics as Reversion, Telegony, The Transmission of Acquired Characters, The Inheritance of Disease, The Biometric Study of Heredity, Mendelism, The Inheritance of Sex

and many others are pretty exhaustively treated, it is remarkable how few they are. The most considerable omission is, perhaps, the absence of any exposition of Dr. Archdall Reid's speculative contributions to this subject. Again, cases cited as non-Mendelian have by now been brought in line with others which are understood.

A. D. DARDISHIRE.

Hints for Crystal-drawing. By MARGARET REEKS. With a Preface by John W. Evans, D.Sc., LL.B., F.G.S. [Pp. 148 + xv.] With 5 figures and 44 plates. (London : Longmans, Green & Co., 1908.)

THE student who wishes to gain a proper comprehension of the aspect of crystals possessing certain symmetry and bounded by particular groups of faces, must acquire some facility in the correct delineation of crystals on paper. As Dr. Evans points out in the preface which he has contributed, models are not readily available, and, in any case, their use would not have such educational value as adequate training in crystal-drawing. Mineralogical text-books devote a chapter to this subject ; but considerations of space permit of only a general discussion, which, because of its brevity and the want of sufficient examples, may prove puzzling to many students approaching the subject for the first time. Such readers should be grateful to Miss Reeks for the thorough and comprehensive nature of what she modestly calls hints.

Pictures of crystals differ from both ordinary perspective and mathematical drawings, and yet embrace the principles of both. To give as complete an idea of the shape of the crystal as possible, something more than a plan is required, and to retain the parallelism of edges, which is an expression of Haily's great law, the plane of the paper must be supposed to be at an infinite distance from the eye. The method generally followed by crystallographers is that adopted by Mohs nearly a century ago in his treatise on mineralogy. The direction of any edge upon the crystal is easily determined as soon as the positions of the fundamental axes and the unit distances upon them have been found. How this important preliminary step may be achieved by purely graphical means, even in the complex case of the triclinic system, is lucidly explained by Miss Reeks. The understanding of the text is greatly facilitated by the excellent plates which have been reproduced from the author's own drawings.

G. F. HERBERT SMITH.

A Study of Splashes. By A. M. WORTHINGTON, C.B., F.R.S. [Pp. 129 + xii.] (Longmans, Green & Co. 6s. 6d. net.)

By the aid of nearly two hundred instantaneous photographs systematically arranged and beautifully reproduced, this book exhibits what happens when a falling sphere meets and passes into a mass of liquid, originally at rest, under various circumstances.

The least eventful case occurs when the sphere is solid and smooth and when the velocity at contact is not very great. Then the liquid rises rapidly round the sphere and envelops it completely, like a "sheath," before it has sunk below the original level of the fluid. When the sphere is rough, the splash does not follow its surface, but rises round it like a bowl-shaped "basket." Afterwards this subsides, and a tall, vertical jet of liquid rises from the bottom of a pit which the falling sphere produces.

If the velocity at contact is great enough, the basket-splash closes upon itself at

the top through the operation of surface tension, and forms a bubble, which may afterwards burst spontaneously or be pierced by the uprising central jet.

If the velocity at contact is still further increased, then, after the basket-splash has closed over the air-tube following in the wake of the sphere, a jet may be projected vertically downwards from the roof of the subsiding splash into the air beneath.

The forces at work near the surface of the sphere as it enters the liquid are beautifully illustrated by a study of the case in which a falling drop of liquid takes the place of the solid sphere.

The book can be understood without any previous knowledge of physics: its text consists for the most part of descriptions of the photographs; but brief explanations of a general kind are given of the way in which the forces at work may produce the observed phenomena. The author thus presents a record, easy to follow and full of interest, of a study in which patience and experimental skill have played a conspicuous part. He adds a new interest to everyday occurrences of which the significance is apt to be overlooked, and supplies the reader with a key to the comprehension of phenomena which must often have seemed mysterious.

S. W. J. SMITH.

Les Zoocécidies des Plantes d'Europe et du Bassin de la Méditerranée.

Tome i. By C. HOUBARD. [Pp. 570, with 824 figures and 2 plates.]
(Paris: A. Hermann, 1908.)

THIS work will be completed in two tomes, the first of which has now appeared. It will prove to be of great service to the student of vegetal galls and their makers, and also to the economic biologist.

It is really a catalogue of all the known galls produced by animals, so far found in Europe and the Mediterranean area.

As far as we have checked it, it seems to be most complete. In this first tome no fewer than 3,319 galls are enumerated. This number, however, includes many of the same species indexed over and over again on the various host-plants, and does not represent the actual number of distinct species of gall-forming animals. To find the actual number of galls produced by various creatures the reader must count up for himself. We hope a complete list of animals which produce these deformities on plants will be given in Tome ii.

The gall-formers and their products are mentioned under the names of the various subjects upon which they occur. This is an excellent method of treating this subject and will enable the student of economic biology easily to trace down any of these structures on cultivated plants.

In the present tome the gall-formers on the Thallophytes, Muscineæ, and Vascular Cryptogams are all dealt with, and those on the Phanerogams up to the Rosaceæ.

In many of the chief families of plants a short general introductory account, printed in small type, is given of their gall-forming enemies. Then each genus and species is mentioned, and the galls and their originators catalogued under each species of plant. A very brief description of many of the deformities produced is given, and references by number added below in small type—references to bibliography—which will, of course, follow in Tome ii.

To show the great number of galls formed by which plants are liable to be attacked we may point out that the author records no fewer than 83 on Conifera of various kinds, 177 on various Gramineæ. The Fagaceæ (Chestnut, Beech, and Oak)

take up the largest portion of the catalogue, extending from page 204 to page 364, 892 galls being mentioned under this family. Of course this includes the same gall-forming animal more than once on different genera and species of the same family. Amongst the gall-forming animals catalogued we find not only insects and mites, but also the parasitical Anguillulidæ or Eelworms.

It is interesting to note the occurrence of the knot root eelworm (*Heterodera radicicola* Greff.) on *Alliaria officinalis*, the garlic mustard, so frequent in Southern England, and on *Lepidium sativum*, one of the cresses.

There are numerous figures of galls, which will prove a help to students.

The references do not seem to be anything like complete, which is a pity in a valuable work of this kind.

The nomenclature of most recent date is wisely adopted; but it would have been of great help if the various synonymic generic and specific names had been given.

The catalogue, in spite of such minor defects, is a most valuable contribution to zoological literature, and must be especially welcome to the economic biologist.

There is a frontispiece, with portraits of Olivier, Bosc, Schlechtendal, and Thomas, and there are 824 figures in the text.

The work is published by A. Hermann, Rue de la Sorbonne, Paris, and the subscription for the two volumes is 40 francs.

FRED. V. THEOBALD.

A Manual of Elementary Forest Zoology for India. By E. P. STEBBING, F.L.S., F.Z.S., F.E.S. [Pp. 229 + xxiv. and 422 figures.] (Calcutta, 1908. 15s.)

THIS most useful volume has been primarily drawn up as a manual for the use of the students at the Imperial Forest College at Dehra Dun. We venture to think it will serve a very much wider purpose and will be used as the standard work by all who study the subject of Indian forestry or who have work in that department.

The manual covers all the animal kingdom from the protozoa upwards, but the part dealing with these minute animals (of no arboricultural importance) and others such as tape-worms, etc. are merely mentioned in the first chapter on "The Scheme of the Animal Kingdom." Chapter II. is devoted to a systematic examination of the Phylum Arthropoda; several additions we hope to see included here in a future edition. There is no mention made of *Eriophyidae*, *Oribatida*, and other acari which may be found of economic importance when more is known of the forest pests of India.

A chapter is given on the general character of insects. Needless to say, these animals claim the major part of the work, extending from page 33 to page 167.

The life-histories are given in small print, at least such as are known, and here and there treatment and remedies are mentioned in notes. Practically all the known common forest insects are dealt with and are well illustrated. We notice some errors in the names, however. For instance, on plate XXIX. and page 88 an *Anthrenus* and its larva is described as *Dermestes vulpinus*, and on Plate LXIV., fig. 285, a male and female *Anopheles* are quite in error, and are apparently a *Toxorhynchites*. Amongst such a large amount of information given one is sure to find some errors and slips.

Chapter XII. onwards deals with vertebrate animals. Numbers of snakes, birds, and mammals of direct or indirect importance to the forester are figured and briefly described, sufficiently, however, to enable the forest student and forester to

identify them. Some of the plates of insects, etc., are excellent, others are somewhat crude, but all are sufficient to allow of the identification of the animal.

This work must prove to be of great service in India, and we hope will be a stepping-stone to a larger and revised edition. In the meanwhile it fills a great gap, and the author is to be congratulated on its production.

FRED. V. THEOBALD.

Gli Insetti. By PROFESSOR ANTONIO BERLESE. (Milan: Società Editrice Libreria, Via Kramer, 4A Gall. De Cristoforis, 54-55.) The price for the parts so far issued is 27 lira.

WE wish to call attention to this great work, started in 1906. At present the first volume has reached Chapter XXX.

It is certainly the greatest work yet produced on insects. The various chapters deal with the external and internal structure of all groups of insects. The last fasciculi, Nos. 28-30, terminate at page 896, and there are 1,197 figures, some of which are coloured. Copious bibliographies are given at the end of each chapter. No student of entomology should be without this monumental work in his library.

We withhold a full notice of this work until the first volume is complete.

FRED V. THEOBALD.

The Fundamental Conceptions of Chemistry. By PROFESSOR JØRGENSEN. Translated by M. P. Appleby. [Pp. viii. + 175.] (London: The Society for Promoting Christian Knowledge. 2s. 6d.)

THIS celebrated little book makes a welcome first appearance in the English language, and apparently has lost nothing in the process of translation. In no way is it to be regarded merely as an elementary text-book of chemistry, but rather as an attempt to correlate the fundamental chemical theories in a manner suitable to the beginner. In this Professor Jorgensen succeeds admirably, the underlying theories of chemical action being explained with clearness, and illustrated by a number of simple experiments, most of which can be carried out with very simple apparatus. The book also makes good reading for the more advanced student and the teacher.

S. J. M. AULD.

On the Plantation, Cultivating, and Curing of Para Indian Rubber. By H. A. WICKHAM. [Pp. 78.] (Kegan Paul, Trench, Trubner & Co. Ltd. 3s. 6d. net.)

ONE of the most noteworthy events in tropical agriculture during recent years has been the attention devoted to the rubber trees, and particularly, so far as the British Possessions are concerned, to the Para rubber tree (*Hevea brasiliensis*). To within the last few years the whole of the world's supply of rubber was obtained from wild plants growing in the great forest regions of equatorial America, and to a less degree in those of Africa and Asia. The mode of collection was exceedingly wasteful and destructive, and about thirty years ago fears began to be entertained lest the natural supplies should become exhausted. At this juncture, owing to the initiative of Sir Joseph Hooker, then Director of the Royal Botanic Gardens, Kew, Mr. H. A. Wickham was commissioned, on behalf of the Government of India, to obtain supplies of the Para rubber tree, the chief rubber-producing plant in the Amazon valley, which then, as now, was the principal source of the world's supplies.

In this volume he tells the story of how, by taking full advantage of very exceptional circumstances, he was enabled safely to bring to England sufficient seeds, which, later, at the botanic garden at Heneratgodia in Ceylon, became the parent stock of the present flourishing industries in Ceylon, British Malaya, India and other parts of the world. It is lamentable to find, owing to the author's original reports not being readily accessible to planters and others, that erroneous ideas about the requirements of the plant obtained credence, and much valuable time was lost in laboriously gaining, by slow and often costly experience, knowledge which was already well known, at any rate to Mr. Wickham. The idea, for instance, that Hevea thrives in swampy ground, although quite incorrect, persisted for a very long time, and even yet has not altogether disappeared.

Concerning the mode of cultivation Mr. Wickham advocates wider planting (33×33 feet) than is as yet generally adopted, and is in favour of setting the young plants in small cleared areas in forest land, without going to the expense, with the accompanying destruction of the humus, of clearing and burning all the standing vegetation as is usual. The author's views on tapping and curing will also be of great interest to all engaged in rubber producing. The book, small though it is, contains a good deal of repetition, but it deals with the subject throughout in a very practical manner; and the author's views, considering his great experience with the plant, both in its native home and also on estates, are well worthy of careful attention, not the least on those points in which he is not in agreement with current practice.

W. G. FREEMAN.

Practical Zoology. By T. J. and W. N. PARKER. [Pp. xii. + 624.] (Macmillan & Co., 1908. 10s. 6d.)

A SECOND edition of this well-known text-book has been issued. An account of *Nereis* and *Monocystis* has been added; and *Obelia* has been substituted for *Bougainvillea*. It is a very serviceable introduction to the study of Zoology, and contains a great deal of valuable information.

But there are certain new points which we think ought to have been alluded to, and some of the illustrations in it might have been omitted.

Gaupp's masterly monograph on the Frog has placed within our reach an account of the anatomy and physiology of the frog's heart which is far in advance of that with which we have had to be content hitherto. Again, the account of the formation of the segmentation cavity in the frog is most meagre. Morgan, in his "The Development of the Frog's Egg," has placed what is known on this subject within easy reach of all. Some account of the development of the mammal and of the homologies of its fetal membranes would have added to the interest of the book.

There are, we think, rather too many diagrams and names printed in italics or black type. Names should not be emphasised. For example, to call the stage before the gastrula of the frog both a *polyplast* and *morula* does not enlighten the student. The elementary student is apt to think that the names are the important things, and in labelling his drawing of a dissection will very often make an arrow going *from* an organ *to* its name, as if the name were the goal. Text-books are largely responsible for this inversion of the real relative importance of the two.

Nevertheless, the authors of this book have, and express, very sound opinions on the relative importance of text-book knowledge and the knowledge obtained from dissection of animals. They also retain what so many zoologists have lost, a

deep interest in physiology, which greatly adds to the value of the work as a text-book for students. If it is true that the volume does not contain one or two things that perhaps it might, it is also true that it contains many valuable things which are not to be found elsewhere.

A. D. DARBISHIRE.

The Mechanical Production of Cold. By J. A. EWING, C.B., LL.D., F.R.S., M.Inst.C.E. [Pp. x. + 204, 71 illustrations.] (Cambridge University Press. 10s.)

THIS book is, in the main, a reprint of the "Howard" lectures delivered by Dr. Ewing before the Society of Arts in 1897, but a good deal of matter has been added and a few corrections made. As the additional matter is chiefly in the direction of explaining the applications of refrigeration and the corrections very, very few and unimportant, we realise, even if the author had not indicated to us, that "in its main features the art of refrigeration has undergone little change" in the eleven years that have elapsed since the lectures were delivered, and yet what a vast field the subject offers for research!

The original form of lectures has been retained, and so, instead of chapters, we have six lectures which occupy the first 180 pages, the remaining 20 pages being devoted to six appendices.

Lecture I. deals with the general principles of refrigeration, and, some small knowledge of thermodynamics being assumed, the "efficiency," or more correctly the "coefficient of performance" of an apparatus for the production of cold, is discussed, it being explained that the term "production of cold" simply meaning the reduction of the temperature of a body below the general level of temperature of its surroundings, and the apparatus employed a "heat pump" or refrigerating machine—the various types of which are described in outline to be dealt with in detail in subsequent lectures.

On page 2 we find: "The commercial importance of mechanical refrigeration is indicated by the fact that there are two journals exclusively devoted to the subject—the American *Ice and Refrigeration* and the German *Zeitschrift für die gesamte Kälte-Industrie*." In keeping with the rest of the volume, a footnote should have been added indicating that the number has now greatly increased, and in England alone we have two important journals, of which *Ice and Cold Storage* should certainly not be overlooked.

Lecture II. deals very thoroughly with air-machines, Lecture III. with absorption machines, and Lecture IV. with the vapour-compression process—ammonia, carbonic acid, and sulphurous acid machines. Very important practical and theoretical comparisons are made between the various types of machines in the latter lecture; and we find on page 86: "The convenient range of pressure of ammonia and the comparatively small bulk of the machine commend it to general acceptance . . . there can be no question that, whatever the merits of other types, the ammonia-compression machine stands easily first as regards the ratio of refrigerating effect to power expended in producing it."

Throughout the first four lectures the importance of the *range of temperature* in estimating the coefficient of performance is kept constantly to the front, and it was, perhaps, hardly to be expected that the author would have added to his work of 1897 by suggesting values for T_1 and T_2 which could be used as a standard range for estimating the performances of refrigerating machines, and yet such a means of rating is one of the wants of this branch of engineering.

Trials of refrigerating machines and the uses of mechanical refrigeration are

dealt with in Lecture V., and in addition to cold storage and ice-making machinery we find a reference to the drying of air for blast furnaces and an ample description, with illustrations, of the methods adopted for cooling magazines in war-ships.

Lecture VI. deals with the applications of extreme cold, and the matter it contains is certainly, from an engineering point of view, the best and most comprehensive treatment we have of the methods and machinery for the liquefaction of "gases" and the production of very low temperatures.

An appendix on the thermo-dynamics of liquid air would have made the matter still more complete, as in the appendices we find further considerations of most of the theoretical points raised in the lectures, including a most important description of the use of Mollier's $\phi - i$ and $p - i$ diagrams (where $i = u + A\phi v$)—diagrams of immense value, for instance, when dealing with the difficult questions that arise when attempting the design of carbonic acid machines for use in warm climates.

In reading such a book one forgets there is such a thing as technical "instruction" and thinks only of "education," and yet not a single technical problem of any importance in mechanical refrigeration is left untouched; and the reader, whether he be a student or practical engineer, will find all he requires in this volume outside of that experience and knowledge which is born only of actual practice.

The mechanical engineering student of to-day is happy in the possession of three such books as *Steam Engine and other Heat Engines*, *Strength of Materials*, and *Mechanical Production of Cold*, all by Dr. Ewing, and happier still the technical institution which uses them as a basis for its own work.

J. WEMYSS ANDERSON.

Notices sur des plantes utiles ou intéressantes de la Flore du Congo. Par ÉMILE DE WILDEMAN. [Vol. ii. fasc. ii. (1908), pp. 167—270.]

THE whole of this fascicle is taken up by an article headed: "APOCYNACÉES. *Notes sur leur valeur caoutchoutifère et leur distribution dans le Congo Belge.*" Under this somewhat misleading title the author includes notes, mostly records of new localities of about thirty-five African *Apocynaceæ*, and a complete enumeration of the species of the genera *Landolphia* (61), *Clitandra* (30), and *Carpodinus* (39). This enumeration, which covers seventy-five pages, contains numerous references to the more recent literature on the subject, many new distribution records, and interspersed here and there critical and sometimes rather lengthy remarks on their specific characters or new observations which were suggested by the collectors' notes and the evidently very rich material of the Brussels herbarium. In several cases amended and extended descriptions are given, without, however, adding much that is new. Not less than ten pages are devoted to *Landolphia ovariensis*, which would appear to be extremely polymorphic, unless indeed the author included several distinct species under that name. As in his *Mission Laurent*, Dr. De Wildeman pronounces himself also here in favour of the cutting method against the tapping of the rubber vines; and here he is in accord with one of the most competent authorities on the subject, namely Dr. Aug. Chevalier, who also thinks that the tapping is in the end more destructive to the vines than their cutting back to some distance above the base. The cumbrous synonymy of the three genera is dealt with in three separate lists, which, like the table of vernaculars, should be very useful, although the latter will have to be accepted with caution.

OTTO STAFF.

Economic Zoology: An Introductory Text-book in Zoology, with special reference to its application in Agriculture, Commerce, and Medicine.

BY HERBERT OSBORN, M.Sc. [Pp. xv. + 490, 269 text figs.]. (New York: The Macmillan Company, 1908. 8s. 6d. net.)

WE had imagined that Prof. Osborn's name was a sufficient guarantee on the title-page of any book for thoroughness and efficiency. A careful perusal of the work before us leaves us, however, disappointed, to say the least.

To couple with a text-book of this character the words Economic Zoology is a misnomer, and a reprehensible one, for the economic side of the science of zoology is treated of in such a casual and perfunctory manner that it is practically useless to the would-be student of that subject. A text-book on Economic Zoology this certainly is not, and has no claim to be regarded as such, whatever claims it may possess to be regarded as an introductory text-book of zoology.

For a really carefully planned and well executed text-book on the subject there is undoubtedly a great demand, but it would be a work as different from the one before us as it is possible to imagine.

The space which the author has devoted to the different phyla, groups, etc., evidences a complete lack of appreciation of the importance of the different divisions from the standpoint of their economic importance, thus twenty-two pages are devoted to the Hydrozoa, seventeen to Echinodermata, twenty-seven each to Mollusca and Crustacea, but such important classes, economically, as Cestodes have twelve pages, Nematodes nine, about one to Acanthocephala, and two to Myriapoda. In the chapter treating of the Insecta the economic portion is meagre in the extreme and not up-to-date, as also that on fishes and birds.

In the review of the orders of the Insects, the Collembola are wrongly included in the order Thysanura.

The nineteen orders recognised might very conveniently have been reduced to twelve or thirteen, whilst the names chosen are not always the happiest of those by which modern entomologists know them.

The descriptions of the different animal phyla are already available in a host of zoological text-books, but careful summaries of the hundred and one problems that are receiving the attention of economic zoologists are as yet very scarce; it is these that we look for in vain, or indeed anything in the way of summaries that would be useful to the student. With the general principles of zoology he is already well provided, but of those of its economic aspects or bionomics he is not, and we regret to state that this work does not help him.

The illustrations are numerous, but not always well chosen, and the author fails in many cases to ascribe the source to the original author. In short, the work gives us the impression of having been hurriedly compiled, with little care and a very indefinite object; as a result it is disappointing as an introductory text-book in zoology, and almost useless as one on economic zoology.

W. E. COLLINGE.

The Transformations of the Animal World. By CHARLES DÉPÉRET. Being the authorised translation of "Les Transformations du Monde Animale." Edited by F. Legge. [The International Scientific Series, Vol. xciv.] (London: Kegan Paul, Trench, Trübner & Co., 1909.)

PROF. DÉPÉRET is one of the most distinguished of living French palæontologists, and he possesses in a high degree that faculty of lucid exposition which is one of the gifts of his race. His book, "Les Transformations du Monde

Animale," published in 1907, is well known as an admirable summary of the present state of knowledge regarding the evolution of the animal kingdom as revealed by palæontological research. The idea of introducing this work to English readers was, therefore, an excellent one, and it is all the more to be deplored that the translation has been entrusted to hands so little competent to deal with it.

The volume is edited by Mr. F. Legge, who dates his preface from the Royal Institution; and, as the name of the translator is not given, it is to be assumed that the editor accepts responsibility for the way in which the translation has been carried out. It is apparent on every page, however, that not even the most elementary knowledge of zoological or geological terminology has been brought to bear on the revision of the text. Everywhere familiar technical names and terms appear in ludicrous and novel forms. "Cœlentaries" for Cœlentera, "Balana" (with the plurals "Balana," "Balanae" and "Balanas") for Balanus, "Branchiopods" for Brachiopods, "the square bone" for the quadrate, "Horned sponges" for Horny sponges—these are examples taken at random from a host of others. Not unfrequently the result is to render passages quite unintelligible. For example, on p. 208 we find, "In the *Halitherium* . . . a small basin hollowed out of a small cotyloid cavity exists . . ." In the original (p. 214) this passage reads, "Dans l'*Halitherium* . . . il existe un petit bassin creusé d'une petite cavité cotyloïde . . ." Again, on p. 329 of the English edition, it is stated that "Cayeux, while examining some narrow cuttings effected in certain pre-Cambrian carboniferous schists, thought he recognised" the remains of Radiolaria. The words rendered by "narrow cuttings" are "coupes minces," which any geologist would at once recognise as microscopic sections. On p. 331 we read of "the presence of colonies of Hydroids related to the Stromatopores, who [*sic*] are Molluscs possessing a conical shell (genus *Chuaris*) . . ." This is offered as a translation of "la présence de colonies d'Hydroides voisins des Stromatopores, des Mollusques à coquille conique (genre *Chuaris*). . ."

Of the footnotes which the editor has not been ashamed to add to Prof. Depéret's work, we have only space to quote one, which is typical of the rest. On p. 315, the word "Roussettes" (fruit-bats), which has baffled the translator, is explained in a footnote, "Fruit-eating chiroptera like the squirrel.—Ed."!

Prof. Depéret may be assured that British palæontologists will join in regretting the indignity which has been offered to his work.

W. T. CALMAN.

The Chemical Constitution of the Proteins. By R. H. ADERS PLIMMER, D.Sc.
[Part I., pp. xii. + 100, 3s. net. Part II., pp. xi. + 66, 2s. 6d. net.]
(London: Longmans, Green & Co.)

VERY properly dedicated to Emil Fischer, this book is issued as one of the series of bio-chemical monographs which are appearing under the editorship of the author and Dr. Hopkins, and one or two of which have already been published.

To the average scientific man it may seem that the title of the present disquisition is a trifle in advance of the times, and that the "Chemical Constitution of the Proteins" is still somewhat a matter for conjecture. Whether this be so or not, there is no doubt that the monograph must take a prominent place in the ranks of compilations of special chemical subjects giving, as it does, in the clearest manner, a full account of one of the most important branches of bio-chemistry, and one which has lately been of special interest owing to the master-investigations which are being carried out in its intricacies by Emil Fischer.

Part I. is devoted in its first section to a general account of the protein molecule and its chemical composition as indicated by the amino-acids obtained from it on disintegration, and in Section II. to the chemical constitution and synthesis of these various amino-acids.

Although of necessity the most incomplete part of the work, Part II. is, perhaps, of the greatest importance, as it is given over entirely to an account of the "Synthesis of the Proteins." Here, again, the title claims more for the contents than it is actually possible to give, but the methods of condensing and linking together amino-acids are gone into fully, and one is treated to an excellent description of the polypeptide synthesis, and a masterly résumé of our present knowledge of the chemistry of these and the other important bodies, which it would be difficult to obtain in any other way.

Both parts are supplied with excellent bibliographies which make it possible to refer to the whole of the original work on any one sub-section, but which are not directly correlated with references in the text.

The book is well printed, but the exterior of all the monographs of this series leaves a good deal to be desired, as the light cardboard covers quickly become discoloured, and show a distinct tendency to peel and tear. A cheap cloth cover would have been much more serviceable.

S. J. M. AULD.

American Chemical Journal. Edited by IRA RAMSEN. [Vol. xl. No. 6 ; vol. xli. Nos. 1 and 2. Baltimore, U.S.A.]

THE publication of this number of SCIENCE PROGRESS almost synchronises with the thirtieth birthday of this well-known American publication, the first number of which was issued by its present editor in April 1879. The value of a scientific journal may perhaps best be gauged by the number of times one has occasion to refer to its past issues, and judged in this way the *American Chemical Journal* has a high claim to the kindly regard of chemists. To English workers it has not infrequently afforded hospitality as a medium of publication in the United States. A useful feature of the journal, from its commencement, has been the section of reports written by recognised authorities, and dealing with recent developments in various branches of pure and applied chemistry. In the first number mentioned above such a report on the "Origin of Petroleum," by Dr. Engler, is reprinted in English from the *Zeitschrift für angewandte Chemie*. The reviews of chemical books are also a good feature of the journal. The original work published in the current number includes "The Action of Chlorine upon Urea," by Dr. Chattaway, "The Regulation of Temperature in the Measurement of Osmotic Pressure," by Messrs. Morse and Holland, "An Electrically-controlled Gas Regulator," by Mr. E. E. Reid, "An Apparatus for the Purification of Mercury," by L. J. Desha, and a paper "On the Relative Ease of Addition in the Alkene Group," by Prof. Michael and Mr. Brunel. The last-mentioned paper will be of special interest to chemists, since it contains a fairly full discussion of recent developments in Michael's "rule of addition," first formulated in 1887.

The Science Year Book, 1909. [Pp. 148 and the Diary portion.] (King, Sell & Olding, London. 5s. net.)

THIS now well-known annual volume—Diary, Directory, and Scientific Summary—is perhaps the most useful book of its kind for the writing-tables of scientific workers. The Diary portion gives a page to each day, and, in addition to notes

of general interest, records such facts as the time of rising and setting of the sun and moon, high water at London Bridge, the planets visible, the constellations on the meridian at 9 p.m., and the highest, lowest, and mean maximum and minimum temperatures for the day.

In the introductory portion (pp. 1-148), in addition to the usual information looked for in almanacks and diaries, there are several distinct features. Astronomical matters are dealt with very fully, and there are useful summaries of the geological formations and of the relationships of the chief families of the animal and vegetable kingdoms.

The physical and chemical notes, and the metrological section, contain many facts often wanted at a moment's notice, and here rendered more readily accessible by the good index with which the book is provided. The general summary by Mr. E. Sharpe Grew, of Science in 1908, summarises in a few pages some of the most striking results achieved during the year, and is followed by a glossary of recently introduced scientific names and terms. Lists of the staffs of public institutions and offices, of the universities, and detailed tables of information regarding the chief scientific periodicals and the scientific and learned societies are given. Amongst the latter those of the Colonies and America are included, but a useful addition would certainly be the chief scientific societies of the Continent.

A biographical directory of scientific men, on the familiar lines of *Who's Who*, is given, prefaced with a table of abbreviations, in which we note the absence of "A.R.C.S.," although it appears frequently in other parts of the volume. So far as we have been able to test it, the information appears to be very accurate and reliable, and as a useful work of reference, in addition to its value as a diary, "The Science Year Book" may confidently be recommended.

INDEX TO VOL. III

	PAGE
ALCOCK, N. H. Chloroform in Use	558
Animal Function, Evolution of (Keith Lucas)	472
ARMSTRONG, II. E. A Dream of Fair Hydrone	484
ARMSTRONG, H. E. The Thirst of Salted Water	638
 Bio-chemistry of Animals and Plants (Rosenheim)	 106
BREND, W. A. Tuberculosis among Animals	53
BULLER, A. H. R. The Destruction of Wood by Fungi	361
BURR, M. The South-Eastern Coalfield	379
 CALMAN, W. T. The Problem of the Pycnogons	 687
CAMERON, A. T. The Transformation of Elements, Part II.: Radium	69
CAMERON, A. T. The Transformation of Elements, Part III.: Helium	222
Camphor, Natural and Synthetic (Houseman)	60
Chloroform in Use (Alcock)	558
Chlorophyll, Chemistry of (Schryver)	425
Cholesterine (Dorée and Gardner)	89
Coalfield, South-Eastern (Burr)	379
Colouring of Birds, Artificial Modification of (Lydekker)	265
CROMMELIN, A. D. C. The Expected Return of Halley's Comet	543
CUNNINGHAM, J. T. The Evolution of Man	192
CUTHBERTSON, C. Gaseous Refractive Indices	273
 Darwin and his Publisher (Murray)	 537
Destruction of Wood by Fungi (Buller)	361
DONCASTER, L. Animal Parthenogenesis	40
DORÉE, C., and GARDNER, J. A. Recent Work on Cholesterine	89
Ductless Glands (Swale Vincent)	410
 Ether-Drift (Rankine)	 21
 FANTHAM, H. B. The Spirochætes	 148
 GARDNER, J. A., and DORÉE, C. Recent Work on Cholesterine	 89
Gaseous Refractive Indices (Cuthbertson)	273
 Halley's Comet (Crommelin)	 543
HATCH, F. H. The Classification of the Plutonic Rocks	244

	PAGE
Heredity and Radium at Dublin (Beach Thomas)	177
HOUSEMAN, P. A. Camphor, Natural and Synthetic.	60
Hydrate Theory of Solutions, Part I. (Lowry)	124
Hydrate Theory of Solutions, Part II. (Lowry)	201
Hydrone (Armstrong)	484
Indigo (Robinson).	575
Isomeric Change, Part I. (Lowry)	616
JOHNSTONE, J. Recent Work on Marine Metabolism	I
JONES, D. W. CARMALT. Vaccine Therapy in Theory and Practice	597
KAYE, G. W. C. Röntgen Rays	299
Life: Physical Condition (Snyder)	579
LOWRY, T. MARTIN. Isomeric Change, Part I.	616
LOWRY, T. MARTIN. The Present Position of the Hydrate Theory of Solutions, Part I.	124
LOWRY, T. MARTIN. The Present Position of the Hydrate Theory of Solutions, Part II.	202
LUCAS, KEITH. The Evolution of Animal Function	472
LYDEKKER, R. Artificial Modifications in the Colouring of Birds	265
LYDEKKER, R. Vertebrate Paleontology in 1908	450
Man, Evolution of (Cunningham).	192
Marine Metabolism (Johnstone)	I
MURRAY, JOHN. Darwin and his Publisher	537
Palæolithic Races, Part I. (Sollas)	326
Palæolithic Races, Part II. (Sollas)	500
Palæolithic Races, Part III. (Sollas)	607
Palæontology, Vertebrate, in 1908 (Lydekker)	450
PARKER, G. D. Sleeping Sickness	657
Parthenogenesis, Animal (Doncaster)	40
Plutonic Rocks: Classification (Hatch)	244
Pycnogons (Calman)	687
RANKINE, A. O. Experiments on Ether-Drift	21
ROBINSON, H. H. Recent Research on Indigo	575
Röntgen Rays (Kaye)	299
ROSENHEIM, O. The Bio-chemistry of Animals and Plants	106
Salted Water (Armstrong)	638
SCHRYVER, S. B. The Chemistry of Chlorophyll	425
Sleeping Sickness (Parker)	657
SNYDER, CARL. The Physical Conditions at the Beginning of Life.	579

INDEX TO VOL. III

715

	PAGE
SOLLAS, W. J. Palæolithic Races and their Modern Representatives,	
Part I.	326
SOLLAS, W. J. Palæolithic Races and their Modern Representatives,	
Part II.	500
SOLLAS, W. J. Palæolithic Races and their Modern Representatives.	
Part III.	667
Spirochaetes (Fantham)	148
THOMAS, W. BEACH. Heredity and Radium at Dublin	177
Transformation of Elements, Part II., Radium (Cameron).	69
Transformation of Elements, Part III., Helium (Cameron)	222
Tuberculosis among Animals (Brend)	53
Vaccine Therapy (Jones)	597
VINCENT, SWALE. The Ductless Glands	410

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